THE

HOMŒOPATHIC TIMES.

A MONTHLY JOURNAL

Of Medicine, Surgery, and the Collateral Sciences.

VOL. VII. NEW YORK, DECEMBER, 1879. No. 9.

ORIGINAL ARTICLES.

THE COLOR-SENSE AND COLOR-BLINDNESS.

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[Written for the Bureau of Miscellany, including Anatomy, Chemistry and Physiology, of the Homœopathic Medical Society of the State of Maryland.]

Personally, except in a very limited degree, I possess no practical knowledge of this subject. For the past two years the scientific world has been so agitated about it, and the results of the investigations have become so numerous and interesting, that I feel no hesitation in bringing it before this society even at this somewhat late day.

Color-blindness possesses a history. The first officially known case was related by Joseph Huddart, of London, in a letter to Joseph Priestly, in January, 1777, now more than a hundred years ago. It referred to a shoemaker by the name of Harris, of Cumberland, Eng., and his brother, the captain of a merchant vessel.

The first accurately described case is that of John Dalton, the celebrated English chemist and physicist. Red blind himself, he studied this defect on his own person, and published in 1794 an exact description of it. After him color blindness received the name Daltonismus. People were astonished to hear, from the communication of this celebrated man, that to him the red of the rose and the blue of the sky appeared as the same color, and that no distinction existed between the bright red of the sealing wax and the green of the turf.

Seebeck was the first to examine a relatively large number of persons, and compare them with one another. As early as 1837 he had collected twelve cases of complete color-blindness, and mentioned, besides, a small number of more or less incomplete cases which formed the transition to typical color-blindness. He saw already how deceivable and uncertain it was to estimate the nature of color-blindness from the *name* of the color which the color blind give to the object. He used principally tinted papers, about three hundred in number, and tested by comparison.

George Wilson, professor of technology in the University of Edinburg, was the first to study color-blindness in its relation to the different occupations of life, and to call attention to the danger of employing color-blind persons in the railroad service and in the marine, as well as in other positions where colored signals must be recognized. Wilson examined a great number of persons, such as soldiers, students, police, etc., and found, among 1,154 persons, 56 color-blind; *i. e.*, 5.6 per cent., or one color-blind among 17.7 persons.

Wilson used tinted papers for testing, and depended upon the name of the color given to the pigment by the person being examined; only those who hesitated in distinguishing red and green were challenged and tested according to the method of Seebeck. According to Wilson, the color blind must renounce painting, weaving, tailoring, chemistry, botany, geology, medicine, etc., and especially, their employment on railroads and in the marine is dangerous, and may occasion loss of life. Wilson did not content himself with calling attention to these dangers alone, but he also endeavored to establish rules for prevention, and proposed that the colored signals in use be retained, and all color blind persons be excluded from the service; or that all persons be retained in the service without regard to their color-sense, and the signals be changed.

The single practical result given by Wilson as the result of his work is the decree of the Great Northern Central Railway Company that their collective employés must possess a normal color sense before their admission to the service.

Dr. A Favre, of Lyons, France, consulting physician of the Paris-Lyon-Mediterranean Railway, has for a long time busied himself with the practical side of the question. He examined for color blindness the employés of the above railroad, also soldiers, sailors, and scholars. The work of Dr. Favre has called forth two new ideas. He directs special attention to acquired color blindness, which, according to him, is of tolerably frequent occurrence, and asserts that congenital color-blindness is not incurable, and can be remedied by the systematic and continued practice of the color-sense.

Still later, this subject has been much discussed in Germany, Sweden, and other European countries by Stilling, Holingrew, Magnus, Cohen, Daae, Donders, and others.

THEORY. It is well known that a luminous body sets the smallest particles of the surrounding ether in wave like vibrations, which extend from it in all directions.

When these vibrations come in contact with our sensory organs, whether in a direct line from the luminous body, or reflected by another body, they produce certain changes which are interpreted in our nervous centres into definite sensations. If they come in contact with our skin, they are interpreted as heat; if with our retina, we experience them as light. So it is our brain which, as the result of certain changes in our sensory organs, generates light and heat; although we designate the external origin by these names, and say that light and heat rays proceed from the glowing body, yet we must distinguish light and heat in an objective sense—which are one and the same thing, namely, vibrations of ether—from

light and heat in a subjective sense, which are sensations of a totally different nature.

If all vibrations of ether were of a similar nature, or if all the elements of our optic apparatus reached in the same way to all vibrations of ether, we would scarcely appreciate different kinds of light: every specific irritation of our optic apparatus would produce a sensation of light whose intensity would depend upon the strength of the illumination, but not its nature. A quantitative difference could be appreciated, but not a qualitative; in other words, different kinds of light, *i. e.*, colors. Physics teach that the vibrations of ether are of different kinds, and that they are distinguished from one another by the rapidity of the vibrations, and the length of the waves. It is owing to these differences that we appreciate differences in quality, *i. e.*, colors.

All quality of light must necessarily depend upon a difference of function of the elements of our optic apparatus. There are two possibilities: Our optic apparatus possesses only one kind of elements, and these are set in different functional activity by the different kinds of vibrations; or there exists in the retina or brain different terminal apparatus, of which each always reacts in the same way, while they are different from each other.

The principle upon which the sensation of quality of light—*i. e.*, color—is explained, is as follows: If one element is excited alone, or if all are simultaneously excited, but one more than the others, we experience the excitation of this one element as quality of light, as colored light, or as color, and call primitive or fundamental color that which corresponds to the element excited. If two elements are excited, or if they are preferably excited, we see colored light, but in a color which corresponds to the mixed color of the two elements excited. This principle permits of the assumption of many fundamental elements and primitive colors. Likewise, from the simultaneous and similarly strong excitation of all the elements no color sensation can originate. We then see light in general, in contradistinction to absence of light, *i. e.*, darkness.

It is evident that when one element is principally excited, while the others are excited to a less degree, the sensation of the specific quality of light will be weaker accordingly as the excitation of the other elements is stronger, because the sensory effect of the irritation corresponds to a mixing of colorless or white light with the specific color of the principally excited element.

The approximate representation of all colors can be produced by the use of three pigments or colored powders: red, yellow, and blue; red and yellow forming the different shades of orange and orange-yellow, blue and yellow giving a variety of shades of green, blue and red all the purple and violet hues. These facts furnished the foundation for the theory of the three primary colors, of which David Brewster was the most ardent defender. He maintained that there are three fundamental kinds of light—red, yellow, and blue—by the mixture of which all kinds of colored light can be produced, as is the case with the pigments.

This theory has been shown to be entirely without foundation. The results of mixing different kinds of light, by Maxwell's disks, is entirely fatal to the theory of Brewster. Color is a subjective sensation, and its laws can be studied only by considering the subjective sensations produced by mixing different kinds of light. The color of any object is due to its property of absorbing some kinds of light and reflecting others. We can see why the result of physically mixing a yellow and a green pigment may be entirely different from mixing yellow and green light reflected from two surfaces, as is done in the case of Maxwell's disks, when the mixing or union takes place in the nervous centres. By mingling two pigments, white light is twice subjected to the process of absorption, and what remains over is the colored light reflected from the painted surfaces; while, on the other hand, the process of mixing colored light is one of addition. This will be sufficient to show why our color sensations cannot be studied with the palette.

The following is the color theory of Herring: Red, green, yellow, and blue are the simple or principal colors. Pure red and pure green (subjectively) mixed give a colorless sensation of gray; likewise, pure yellow and pure blue. Pure red, on the one hand, forms all conceivable tones of color through yellowish red and blue red, in which we have the sensation of both colors, or recognize them; pure green, on the other hand, forms all conceivable tones of color through yellowish green and blue green, with which both colors are likewise recognizable. Herring indicates red and green, not as complementary, but as antagonistic colors; they work against or destroy each other in the act of exciting the sensation of color, as is also the case with yellow and blue.

Our sight substance is composed of three different kinds of matter, namely: 1. The substance for the sensations from black to white; 2. The substance for the sensations from red to green; and 3. The substance for the sensations from yellow to blue; and are called the black-white, the red-green, and the yellow-blue substances. No color or tone of color can be experienced in complete purity; and, also, the black-white substance is always affected by all the objective colors. If the sight substance is affected by objective yellow and blue in such proportions that the color sensations destroy each other, there remains of them their action on the black and white substance, and the resulting sensation is a colorless gray. The same is true of red and green. From the action of the collective rays of the spectrum, the sensations red and green, yellow and blue, are lost, and there remains only the action on the black-white substance.

The red-green substance is affected, on the one hand, from the most external red of the spectrum to pure yellow; on the other hand, from greenish yellow to blue; and again, by the red contained in the violet of the spectrum; in pure yellow and in pure blue, not at all; in pure yellow, certainly not, because the action of red and the antagonistic action of green destroy each other; in complete blue not, because the action upon the green-red substance occurs with the opposite action of the violet upon the green-red substance. The yellow-blue substance is excited, however, from red to green, and from blue green to the end of the spectrum. (Aubert. Physiologische Optik. Farben-Sinro. Graefe und Saemisch, Bd. II.)

According to the above theory, color blindness must consist of an absence of or want of function of one or the other color-seeing substances; and color-blind persons must be either red-green or yellow-blue, or, finally, totally color-blind. With red-green blindness the color system consists only of yellow and blue. The spectrum appears only in these two colors. Colored stuffs appear yellow, blue, or gray, according to the quantity of the different kinds of reflected light. The spectrum is, in most cases, of normal length; in single cases, much shortened.

Blue-yellow blindness. The color system consists only of red and green. The spectrum appears only in these two colors. Colored stuffs appear green, red, or gray, according to the relative quantities of the reflected kinds of light. The spectrum can be much shortened, and it can be of normal length.

In comparison with red-green blindness, blue yellow blindness is very rare. Total color-blindness is relatively very rare, and little understood. They see only black, white, and gray.

Early in the present century Thomas Young first propounded the theory of three fundamental colors.

About 1850 this theory was reviewed and advocated by Helmholtz, since which time it has received many adherents, and is known as the Young-Helmholtz hypothesis. It assumes three fundamental colors; red, green, and violet, and, accordingly, three corresponding elements in the optic apparatus. If the red sensitive elements are alone excited, or more than the others, we experience red, and so for the others. The different vibrations of ether excite the different elements in a different degree, but always so that all are set in a certain degree of excitement Homogeneous red light irritates, from the outer end of the spectrum to its side of orange, the red sensitive elements in a high degree; much less the green, and still less the violet. In this way the sensation of red originates, which goes more and more into orange as the irritation of the green sensitive elements increases. Homogeneous yellow irritates strongly, and, in almost the same degree, the red and green sensitive elements, while it affects the violet sensitive elements only very weakly. On this account the sensation of yellow is a combination of red and green. Green light irritates the green sensitive elements strongly, the two others very weakly and in almost the same degree Hence the sensation of green. Homogeneous blue irritates tolerably strong, and in almost the same degree, the green and violet sensitive elements; but the red in a less degree. Hence the sensation of blue is a mixture of green and violet. Violet light affects the violet sensitive elements actively, but the two others less. Hence the sensation of violet.

According to this theory, color blindness must consist of red, green, violet, or total color blindness, according as one, the other, or all of the color sensitive elements are absent, or are deficient in functional activity.

Red blindness originates from the absence of or want of functional activity of the red sensitive elements. The red blind possess two fundamental colors, namely, green and violet. To the red blind, spectral red appears as a saturated dark green; yellow, as a light saturated green; green, as a light but whitish gradation, of the same color as red and yellow; blue, as blue; and violet, as violet or dark blue.

Green blindness consists in the absence of or want of functional activity of the green sensitive elements. The green blind possess two fundamental colors, viz.: red and violet. The green blind see the spectral red as a dark but much saturated red; yellow, as a light red; green, as white or gray; blue, as a color similar to the indigo; and violet, as a much saturated violet.

Violet blindness consists in the absence of or want of functional activity of the violet sensitive elements. The violet blind possess two fundamental colors, red and green. They see the spectral red as red; yellow, as white or gray; green, as blue green; blue, as green; and violet, as dark green.

CLASSIFICATION OF COLOR-BLINDNESS.

Holmgren classifies color blindness as follows, in harmony with the color theory of young Helmholtz:

I. Total color blindness; where the ability to see colors is totally wanting, and where the sense of sight is able to distinguish the difference between light and darkness, with their different grades of intensity.

II. Partial color blindness; where the ability to distinguish definite tones of color, not all, fails. It is divided into—1. Complete or typical color blindness; where one of the three fundamental color sensations, *i. e.*, one of the three color sensitive organs, fails. This group includes three kinds, viz.: *a*, red blindness, *b*, green blindness, *c*, violet blindness. 2. Incomplete color blindness; where one or all three of the elements are weaker in irritability or number than the normal color sense. This group includes, *a*, a higher degree, which can be indicated as incomplete red, green, and violet blindness; and *b*, a lower degree, which may be indicated as a weak color sense.

Stilling classifies color blindness, accordingly, as those color sensations which are indicated as simple, are absent, and in harmony with the theory of Herring, of four principal or simple sensations. Since each two of these stand in a polar relation as positive and negative, so, when one color fails with a color blind person, the other antagonistic color must also fail. Either the collective color sensations fail, or the sensations of red and green, or yellow and blue, fail at the same time.

1. Color blindness is divided into
   - *a*. Red-green blindness.
   - *b*. Blue-yellow "
   - *c*. Total color "

2. Decreased color sensitiveness is divided into
   - *a*. That for red-green.
   - *b*. That for blue-yellow.
   - *c*. Total.

These different kinds of decreased sensibility of the color sense are not sharply separated. Not seldom true blindness for two antagonistic colors is united with a decreased sensibility for the two others, forming a transition between partial and total color blindness.

Color blind persons possess a great sensitiveness toward light effects, which compensates them in some degree for their loss of color effects. It enables them to experience fine and delicate shades of light when a color-seeing eye would remark nothing. They also understand how to make their fine power of distinction for differences in light of practical value. It enables them to distinguish colors from one another, which, on account of their color blindness, seem alike. They discover that colors, which to them falsely appear identical, do not possess a like amount of light, and they use this difference in the amount of light to distinguish color tones from each other. This power is much developed by practice. Magnus mentions the case of a red-blind locomotive engineer who understood so well how to hide his color blindness, that, in a service of ten years, he did not once mistake the red signal, nor confound it with the green, and was supposed by his superiors, on this account, to be normal sighted. His defect was first brought to light by the obligatory examination of the color sense of the railroad employés. Magnus frequently convinced himself that this man did not confound the red and green signals, in spite of the fact that the examination of his color sense showed him to be in a high degree red-blind. He afterward gave this author to understand that he was aware of his color blindness for a long time, and in distinguishing between the red and green signals, he had been helped only by the amount of light possessed by the two colors. Magnus also relates an interesting case of a red-blind painter's apprentice, who had been taught by a master for three years, without the master having any suspicion of the color blindness of his assistant. This individual had been able to strike the distinction between the different colors only by his sharpened sensations of light effects. He was so agitated by his condition, and his anxiety that he would finally commit an error so frightened him, that he confessed his failing, and gave up his vocation as a painter.

The opinion of Dr. Favre, the French railroad physician, that color blindness can be removed by a long and systematic practice of the color sense, has not been confirmed by experience; and, indeed, the evidence is that the condition of the congenitally color-blind cannot be improved. This is clearly evinced by numerous cases of color-blind individuals who, through long years of practice, were never able in any way to improve their condition. For example, the case of Harris, the shoemaker, and the celebrated John Dalton. Harris was aware of his defect from his fourth year,

and Dalton studied his own defect for many years; neither of these persons were able to improve their condition. It seems unnecessary to mention the painter's apprentice, who, after three years' practice in colors, gave up his calling on account of his color blindness. "Therefore a systematic exercise of the color sensations of the color-blind is a perfectly useless undertaking." (Magnus.)

Color-blindness is congenital, and, in some degree, hereditary, as demonstrated by numerous examples, and affects almost exclusively the male gender. The example of the shoemaker, Harris, shows that color-blindness is a family failing, though rarely affecting all members. Harris had two brothers, who, like himself, were color-blind; however, besides, there was one brother and a sister with a normal color sense.

Horner contributes two interesting pedigrees, extending over several generations. From the one it is seen that five daughters of a color-blind individual did not suffer from color-blindness; it affected, however, their collective male descendants, with two exceptions, in the five following generations, while all the females remained free.

The other, the pedigree of a color-blind individual born in 1642, showed only male color-blind descendants to the seventh generation, and always so that the color-blindness was transmitted through a normal-seeing female member to her male descendants.

Color-blind individuals are naturally incapacitated for many callings in life, and their employment in positions where colored signals are to be recognized, as in the railroad service and the marine, is positively dangerous, and has probably occasioned both loss of life and property. Therefore, the necessity of a careful, uniform, and repeated examination of the color sense of the employés of railroads and marine is self-apparent. Although color-blind persons have served in the capacity of railroad engineers without coming to grief, being able to distinguish between the different signals by their acute perception for different light effects, yet the conditions modifying light are so numerous and variable as to render this a very uncertain and hazardous method of deciding.

To Dr. Favre belongs the honor of having first directed special attention to the importance of repeating the examinations at regular intervals, in order to detect cases of acquired color-blindness arising among employés from bodily causes.

The color scale of the color-blind is much more penurious and poorer than that of the normal sighted. A considerable number of colors which appear to the normal color sense as entirely different, and belonging to different groups, and to which different names are applied, have for the color-blind no distinction. They appear to the color-blind as identical, or as so similar that he is unable to make a specific distinction between them. Therefore, the system of colors in use by the normal-sighted is useless to the color-blind, and not understood by them, and they can never learn to understand the color designations in use by the normal-sighted, or apply them to their abnormal color sensations.

For these and other reasons in testing for color-blindness, methods which are based or depend on the subjective statements of the person to be examined must be avoided. In naming colors, the color-blind by practice may acquire much dexterity, and thus avoid detection; and the unpracticed, yet normal, color sense can, by the subjective statements be classed as abnormal.

Tests for color blindness. The tests for color blindness are numerous; some of them time-consuming and difficult of conducting, while the necessary apparatus is complicated and expensive. I shall limit the remarks to three tests which are characterized by their simplicity, effectiveness, ease of performance, and—at least, two of them—by their adaptability to testing large bodies of persons.

The first test I shall describe is that of the colored shadows, or simultaneous contrast, of which Dr. J. Stilling is the most enthusiastic supporter. If a white surface is illuminated by two lights, one of which is colored, and some object, such as a pencil, be brought between the lights and the illuminated surface, two shadows will be cast on the surface, one of which will be of the same color as the colored light; the other will appear in its antagonistic or complementary color. This appearance has never been satisfactorily explained as yet. Stilling says: "All that can be said is that, under certain circumstances, a colored illumination *forces* the normal eye to produce subjectively upon the shadow the opposite or antagonistic color. By presenting to the eye lights of all the different colors, it can be forced to produce subjectively all the colors it is capable of appreciating. An eye insensible to any given color will not produce upon the shadow its complementary color, and instead of appearing colored, as to the normal eye, the shadow will appear dark, provided the inducing color act alone." To produce shadows for this purpose, allow the light of a lamp to shine through a colored plate of glass upon a sheet of white paper fastened to the wall; then interpose an ordinary lead pencil, which will throw a single shadow in the complementary color. For ordinary purposes, two lamps are unnecessary, as, in case the room is papered with light paper, sufficient light for the purpose will be reflected. If the wall paper is dark, a second white sheet of paper may be used for reflecting light upon the first, or ordinary daylight can be admitted to the room. For the inducing illumination, four large plates of glass are used, which transmit simply and unmixed the four principal colors.

The large number of experiments on the color blind made in accordance with this method has produced uniform results. "The shadows either appear colorless and dark by red and green illumination, and colored by blue and yellow, or, on the other hand, colorless by blue and yellow illumination, and colored by red and green. In some cases no color would be perceived in any of the shadows." When two lights are used, forming two well defined shadows at the same time, either the objective red and the subjective green, or the objective blue and the subjective yellow appear colorless, or *vice versa*.

According to these experiments, the sensations red and green, or blue and yellow are at the same time wanting, or in some cases all color perceptions are absent, or very slightly developed. The contrasts can be made weaker or stronger according to the intensity of the lights, or the more or less marked prominence of the inducing color. This test could be further amplified, but sufficient has been said to show the principle upon which it is based. It is scarcely necessary to add that it is not adapted for examining large numbers of people when rapidity is of paramount importance.

Holmgren's test, which is simple, complete, and easily and rapidly executed, depends upon the principle of comparison. A supply of colored worsteds in the colors red, orange, yellow, yellow-green, green, blue-green, blue, violet, purple, rose, brown and gray, several shades of each color, and at least five degrees of saturation, is necessary. Green and grays of several kinds, red, blue, and violet, and shades of light brown, yellow, red and rose must be especially present.

The collection of worsteds is spread upon a table, and the examiner selects the color he will use for the test from the collection, and lays it on one side; he then requests the subject to select from the collection those colors which seem to him to come nearest the test, and lay them by its side. According to the kind of colors, and the manner they are selected, the color sense is exposed.

For diagnosing a deficient color sense, green is selected as the test color, because green is the lightest

of the spectral colors, and therefore is the easiest confounded with gray. For diagnosing different kinds of complete partial color-blindness, purple (rose) is selected, *i. e.*, the entire group of colors in which red (orange) and violet (blue) unite, and in which neither of these colors preponderate. Although purple is a compound color, it is completely saturated, and on this account it has been called the eighth spectral color. For the examination of the color blind, purple is of great importance, because it is formed by the union of the two colors found at the extreme ends of the spectrum, and which are never confounded with one another. On this account, and according to the theory of Young Helmholtz, purple with the color blind either excites only one of the color sensitive organs, or it excites both simultaneously. Accordingly, it appears only as a simple color, *i. e.*, as one of the two of which it is composed, or as white (gray).

Test 1. The examiner lays before the subject a pure, light green bundle of worsted. The examination must be continued until the subject has laid beside the test all the bundles of worsted of the same color; or, until with these, also gray, reddish, and rose-colored worsteds. Those who lay beside green, also reddish, gray, and rose colored worsted with the test, are color blind; those who, without being guilty of this error, show a decided inclination to it, have a weak color sense.

Test 2. A purple (rose) colored worsted, in a shade midway between the lightest and the darkest, is placed before the subject.

1. Those who are found color blind according to test 1, and who with this test select only purple colored worsteds, are incompletely color blind.

2. Those who with test 2 select entirely, or with purple medium and dark blue and violet, or one of these two colors, are completely red blind.

3. Those who with test 2 select exclusively, or with purple, green and gray, or one of these colors, are completely green blind.

Test 3. A bright red bundle of worsted is laid before the subject. The red blind select, besides the red, green and brown, shades which to the normal eye appear darker than the red. The green blind, on the contrary, select such shades of green and brown as to the normal eye appear lighter than red.

Violet-blindness is betrayed by the confounding of red and orange with the purple of test 2.

Holmgren has since simplified his test as follows:

1. With test 1 all the green tones are removed from the collection, with the single exception of those to which test 1 belongs, and which shall consist of five different shades.

2. When test 2 is to be used, the blue greens of different shades are to be added to the collection.

3. Before commencing the examination, the subjects are to be stood in a circle around the table on which the collection of worsteds lies, so they can all see what occurs.

4. The examiner holds the five green tests before them, and requests them to impress the colors so they will be able to find them with certainty and rapidity when they are mixed with the other colors. Then the examiner lays the first test color, *i. e.*, the lightest of the five green shades, on one side, and mixes the remaining four with the collection. He then immediately selects them from the collection, and lays them with the test; *i. e.*, he executes the test before the eyes of the subjects as they shall execute it. This can be repeated as often as seems necessary for the proper understanding of the test.

5. After this preparation, the subjects are to approach the table one at a time, and repeat the test. The remainder must remain quiet, and not signal by laughing or otherwise, when the person under examination scatters or otherwise disturbs his test.

6. When a person reveals himself as color-blind with test 1, he is immediately subjected to test 2, for the differential diagnosis. Test 3 need not be made.

7. As the test in general, according to the above, proceeds very rapidly, the weak color sense is made known by the slowness of the manipulation.

With this method, more than one hundred disciplined persons can be examined in an hour.

The third method of examining is that with the so-called pseudo-isochromatic plates of Dr. J. Stilling. It consists of a number of plates, principally for the discovery of red-green blindness; several are exclusively intended for the finer examination of discovering a decreased color sense for red and green. There are also similar plates for discovering blue-yellow blindness. The pseudo-isochromatic plates are constructed as follows:

The different shades of both interchangeable colors are intermixed in smaller or larger squares in such a manner that the squares of the one color form letters and figures, and those of the other the ground work, so that the different intensities alternate in ground and letter. In this way every distinction is rendered impossible, and the question as to the judgment of colors is rendered unnecessary, the inquiry being merely about letters, numbers, figures.

By means of these plates, reduced color sense, as well as color-blindness, may be diagnosticated, and it is immaterial whether daylight or lamplight be used, as the hue will suffer no change by the latter. Stilling says: "I believe I am justified in asserting the conviction that, for the estimation of the sensitiveness for red and green, the practitioner posseses in the present plates an apparatus so complete that he has no necessity to use other means of help; at least in the sorting of colored bundles of worsteds, in which the color-blind can become quite expert, and which, on this account, is not certain."

The manner of using the plates and forming the diagnosis consists in holding them before the subject, or giving them into his hands, and requesting him to form (rose or red colored) letters out of the quadrants. According to their ability or inability to decipher the plates, their color sense is revealed. The author believes he has so far improved these plates in relation to lustre, etc., that they can be used by the unpracticed, without any preparatory rules, in any kind of light.

These tests have respectively active opponents, as well as warm admirers, and the final value of each is yet to be determined.

STATISTICS. The per centage of color-blindness, as given by the different examiners, varies very much; while one gives it between three and four per cent., another gives it as ten per cent. and over. The cause of this great difference may be sought in different factors. A pure objective method of examining will give a less per centage than when the subjective statements are considered. Again, the transition of color-blindness to a normal color sense is so gradual that no absolute line of demarkation can be drawn. One examiner may include an individual as color-blind to whom another would attribute a normal color sense. Finally, the material itself is subject to many variations in relation to race, social position, gender, etc.

The statistics of Holmgren have the greatest value. He gives the per centage as 3.25, supported by a material of 32,165 persons examined.

		Per cent.
Dr. Lederer,	in Poland, has found	1.14
Dr. Hansen,	" Copenhagen, "	2.87
Prof. Holmgren,	" Upsala, "	3.25
Dr. Magnus,	" Breslau, "	3.27
Prof. Cohen,	" " "	3.6
Prof. Pflüger,	" Bern, "	3.6
Dr. Jeffries,	" Boston, "	5
Dr. Stilling,	" Cassel, "	5
Dr. Krohn,	" Finnland, "	5
Prof. Wilson,	" Edinburg, "	5.6

		Per cent.
Prof. Donders,	in Utrecht, has found	6.608
Dr. Féris,	" France, "	8.18
Dr. Favre,	" Lyons, "	9 33
Dr. Daae,	" Kragerö, Norw'y	10.24

This material includes persons in different conditions of life, such as railroad employés, soldiers, sailors, police, students, etc.

Dr. Magnus has studied color-blindness in relation to different grades of society with the following results:

Among 2,002 scholars of gymnasia and higher preparatory schools, examined by him, were 53 color-blind. This gives 2.65 per cent. Among 1,035 scholars of grammar and elementary schools were 46 color-blind. This gives 4.36 per cent.

Prof. Cohen found among 1,424 scholars of gymnasia and higher schools, 47 color-blind. This gives about 3.35 per cent. Among 1,005 scholars of grammar and elementary schools, 48 color-blind. This gives about 4.8 per cent.

The following table, given by Prof. Holmgren, is of special value in this relation:

	No. Examined.	No of Color-Blind.				Per Cent.
		Complete.		Incomplete.	Total.	
		Red.	Green.			
Public Scholars ...	3,654	36	51	79	166	4.54
Elementary Scholars	8,682	83	72	145	300	3.45
Students.....	1,523	8	13	26	47	3.08
Young people of different conditions	555	6	7	12	25	4.50
Railroad personél ..	7.953	45	48	78	171	2.15
Seamen..... ...	4,225	22	30	42	94	2.22
Soldiers	1,851	13	20	29	62	3.54
The Arts.....	649	9	4	18	31	4.77
Prisoners	321	5	4	9	18	5.60

Magnus says: "Though these numbers are not sufficiently great to come to any final conclusion, still they show that color-blindness in general is greater with the lower classes than with the higher." He seeks the explanation as follows: With the lower classes a development of the color sense finds no consideration. The children frequently grow up without receiving any idea of the colors or their names. Even if, with the higher grades, the children receive no direct development of the color sense, yet their attention is turned to colors in a greater degree than is the case with the children of the lower classes. Colored toys, picture-books, paint boxes, and the like, are, to the children of the better classes, well known apparatus, and that these colored objects can exert a certain influence upon the development and sharpening of the color sense is generally conceded. The slight inclination to color-blindness shown by the female sex is, perhaps, due to a similar cause. He further says: "The preference given by the cultivated and better classes to the less pronounced and softer colors, while the lower classes love the brighter colors, tends to show that the lower classes have a less developed color sense, and tend more to color-blindness."

I think it not out of place to attempt to show that in some degree this argument is fallacious, and I think the greater inclination of the lower classes to color blindness, as shown by the statistics, is more apparent than real. Every one will concede that the color sense of the lower classes is much less practiced than is the case with the higher classes; this fact alone cannot justify the assumption, even if apparently supported by statistics, that the lower classes incline more to color blindness, which is a congenital and incurable defect. The lack of cultivation in general and want of practice in colors with the lower classes would render them very much more liable to be accidentally classed as color blind—when they lack only intelligence and an education in colors—than would be the case with the cultivated; while the culture itself would assist the higher classes in hiding true color blindness.

Again, as color blindness is congenital and in some degree hereditary, any social condition which could exert an influence on it must exist for a very long time; generations, perhaps. It is well known that the state of society is continually changing; the ancestors of many who are to-day in the higher classes belonged to the lower, and the reverse. Even with the extremely low, the conclusions of Magnus could hardly hold good, as their ranks are continually recruited from the other classes, to which their children in turn return. And here the factor of doubtful parentage is greater than elsewhere.

The opportunities for the color education, as here understood, of the children of the middle classes is not surpassed by any. Colored toys and the like are their constant companions; their parents have been employed with colors in the arts for generations, as they in their turn are.

The fact that the higher classes and more cultivated give the preference to less pronounced and paler colors, while the lower classes love the more brilliant shades, would show that the color sense of the lower classes is less refined, but not that they incline more to color blindness, as assumed by Magnus. In fact, Dr. Burnet, of D. C., has found the percentage of color blindness strikingly small with the negroes, and their preference for loud, bright colors is well known. It has been generally stated that the Germens are much addicted to bright and inharmonious colors, nor have they been shown to possess more color blindness than other people.

There is no longer any doubt that the female sex possesses a more acute color sense than the male, and consequently inclines less to color blindness; a fact mentioned by ophthalmologists in the early part of this century, which then had more the character of a supposition than actual knowledge. At the present day the question is solved beyond doubt.

Prof. Dor found in Berlin, among 611 girls, 5 color-blind.....	0.82 per cent.
Dr. Hansen found in Copenhagen, among 1,025 girls, 0 color-blind	0. "
Dr. Jefferies found in Boston, among 1,025 girls, 0 color-blind.....	0. "
Prof. Cohen found in Breslau, among 1,061 girls, 0 color-blind.....	0. "
Dr. Magnus found in Breslau, among 2,216 girls, 1 color-blind.....	0.04 "
Dr. Burnet found in D. C., among 1,691 girls (colored), 2 color-blind.....	0.11 "
Prof. Holmgren estimates the per cent. among females as.....	0.26 "

The explanation of this extraordinarily small per cent. of color blindness among females, in comparison with males, is sought in the early education of the color-sense by the use of colored objects, stuffs, worsteds, etc., which is peculiar to the female world. Von Bezold says: "With women a difficult color-sense is less frequent than with men, so that we will not be wrong when we ascribe to the female gender in general a more completely developed color-sense. Shall the interest in the toilet, appearing so early with the female, contribute anything to the development of this color-sense?" Lederer remarks: "It is easy to assume that females who are accustomed from childhood to think of the much-colored decoration of their toilet, in this way become and remain skilled in the distinction of colors." Magnus appropriately adds: "Naturally a practice of this kind would never be able to simply heal the one female individual of congenital physiological color-blindness. We dare not attribute to it more than a means of exercising the color-sense, which gradually profits, and its influence does good

not so much to the single individual as to the entire gender."

The color sense of the single individual is gradually and imperceptibly strengthened and educated by early and continued practice. This development of the color sense, insignificant for the single individual, is transmitted from one generation to the other, and becomes a pronounced sexual peculiarity of the female gender. Gradual practice and transmission are the two active factors which help the female to their peculiar preponderance of the color sense over the male. (Magnus.) Holmgren also advances a similar theory: "If practice can have an influence upon this point (less occurrence of Daltonismus with females), according to our view, the single individual will not be healed so much as the entire gender, and in this manner the practice is transmitted unobserved to the coming gender. We know too little of the laws of transmission in order to speak upon such conjectures; however, we definitely know that good as well as bad peculiarities are transmitted. To the latter belongs color blindness, and we believe that practice, in one sense, in the way of transmission, may exert good results, even if we are unable to prove this."

For the relief of color blindness Magnus proposes the early education of the color sense of the male gender by the systematic and methodical practice of this sense in the schools. As already mentioned, this was proposed by Dr. Favre, of France. He proceeded, however, from the supposition that color blindness is a curable defect, and that this was the most certain way of relieving it. Magnus proposes to educate, not so much individuals as the entire gender, and thus produce an improvement in the color sense of the male by transmission, as is supposed to be the case with the other sex. Magnus has made this point an object of study, and constructed a color system for the use of schools. It consists of a color map for the wall of the school containing in eight rows thirty-two different shades of color.

The first color of each row is always a dark and much saturated tone, while the other three of the same row represent lighter shades of the same color. It contains in the first row brown, then follow purple, scarlet, yellow, green, blue, violet, and finally black. To this color map belongs a case containing colored worsteds—three bundles of worsted for each shade of color of the map.

The map is used as follows, the method of instruction being divided into two divisions, one for the smaller (primary), the other for more developed children:

With the first division the color map alone is used, the instruction beginning by the teacher naming the single colors of a row to the children, which they repeat. After which, the teacher points to any color field and requests the children to name the color. In this manner the children learn, not only the names of the characteristic saturated colors, but they will also be enabled to distinguish the lighter shades.

When the children are sufficiently instructed in the first division, the second is brought into use. The teacher shows the scholar a color on the map, and requests him to select the corresponding color from the worsteds. It is best to begin with the more saturated characteristic colors and gradually proceed to the weaker shades. When the scholar has become expert in this procedure, the teacher names to him some colored object, as the violet, forget-me-not, rose, etc., and requests him to select its color from the worsteds, or point it out on the map.

Magnus remarks: "The systematic education of the color sense of individuals who are born color blind is not considered; it only aims at gradually elevating the capacity of the color sense of one generation by systematic practice, and thereby transmitting to the coming gender a more capable color sense, and consequently limiting in this way the extension of color-blindness."

In the earlier times color blindness was considered peculiar to the Germans; later statistics have shown the error of this supposition. It has been stated that a robust constitution and a bilious, melancholic temperament is especially predisposed to color blindness. Also that the color of the hair and eyes stood in some relation to the frequency of Daltonismus, and that blue eyes and blond hair were connected with a pre-disposition to color blindness. All of which theories have been dispelled by experience. Niementschek ten years ago examined four color blind with a pupilary distance of 49.5 to 54 mm. In consequence of this short distance, he supposed there must exist a shortening of the parts between the orbits; the sensation of color lay in the anterior convolutions of the cerebrum; and that it must be narrowed here. The investigations of Cohen show that "this theory is likewise absolutely false."

The examinations of Magnus and Cohen, independently of one another, have shown that with the Jews of Breslau the inclination to color blindness appears to be greater than with the Christians. Among 3,278 boys examined by Magnus, 2,509 were Christians and 764 Jews; among the 2,509 Christians he found 71 color blind, — 2.83 per cent.; among the 764 Jews he found 29 color blind, — 3.79 per cent. With two exclusively religious Jewish schools he found, among 216 scholars, 11 color blind, about 5.09 per cent. This increased tendency to color blindness was confined to the males, no color blindness being found among the females. The results of Cohen's examinations completely confirm those of Magnus. The relation as given by Cohen is as 4.8 : 3.6 per cent.

Certain authors who believe that color sensations originate by a gradual development, regard the greater inclination of the Jews to color-blindness as indicative of a retarded development.

Magnus, who is a believer in the theory of the gradual development of the color sense, seeks the explanation in another factor. As color-blindness is hereditary, and its propagation occurs through transmission, it is natural that the Jews, who for hundreds of years did not mix with the other races, and always marry among themselves, should incline more to the hereditary force of this defect than the Christians.

Dr. Burnet, of D. C., who examined 3,040 colored school children, found among 1,391 boys, 22 color blind, —1.6 per cent. Among 1,691 girls, 2 color blind,—0.11 per cent. The smaller percentage of color-blindness as found among colored boys is certainly very remarkable. Cohen found an excellent color sense with eleven Nubians examined by him, with whom words for the designation of the colors failed, an interesting fact, as Geiger, Magnus, and Gladstone concluded, from the absence of certain color designations from the ancient Greek literature, that the color sense with these people was rudimentary. I shall close with the following recapitulation taken from Jeffries' article on "The Dangers arising from Color-blindness."

Certainly one in fifty, much more probably, one in twenty in the community is color blind. Of this defect they may even themselves be wholly unconscious.

This color-blindness may practically be regarded as red-green blindness and blue-yellow blindness. Total color-blindness also exists.

This defect is congenital. It exists in varying degrees. It is largely hereditary. It may also be permanently or temporarily caused by disease or injury.

It is incurable when congenital. Exercising the eyes with colors, and the ears with their names, helps the color blind to supplement their names, but does not change or increase their color perception.

Experiment and experience show that we are forced to use red and green marine lights to indicate a vessel's direction of motion and movements, and at least red light on railways to designate danger. Form instead of color cannot be used for these purposes.

There are many peculiar conditions under which employés and marines perform their duties which render colored signals, especially colored lights, difficult to be correctly seen. These signals can never be correctly seen by the color blind.

There is, therefore, great danger from color-blindness. Railway and marine accidents have occurred from it. There is no protection but the elimination from the *personel* of the railway, and the vessels, all persons whose positions require perfect perception, and who fail to possess this. This can now be readily and speedily done.

Therefore, through the laws of the legislature, orders from State railroad commissioners, or by the rulers and regulators of the railroad corporations themselves, each and every employé should be carefully tested for color-blindness, and refused if he has it.

Every employé who has had any severe illness, or who has been injured, should be tested again for color-blindness before he is allowed to resume his duties.

The same examination should be carried out among pilots, masters of steamers and sailing vessels. These latter should be especially instructed how to detect color-blindness among the *personel* of their commands.

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—VANILLA BEAN is said to contain an essential oil liable to become rancid and poisonons. Several cases of severe gastro-intestinal disturbances, from eating food flavored with vanilla, are reported.

CROUP DIFFERENTIALLY CONSIDERED—CHLORAL HYDRATE, ETC.

BY ALLAN MOTT KING, M.D., ST. JOHN, NEW BRUNSWICK, CANADA.

I would call attention to a very prevalent belief, held by many medical men, that genuine croup is always attended, nay produced, by the formation of a false membrane, either in the larynx or trachea.

I started out in my professional career fully impressed that this was the correct pathological condition, and invariably formed when death had occurred from croup; and it was some years before I became convinced, from my observations, that children frequently have croup and die from it where no membrane of any nature, except an exudation of mucus, was present.

Our opportunities for post mortem examinations in case of death from croup are very few in private practice, as the friends are naturally averse to having their little ones disturbed after death. But in searching through the few records at my command of post mortem examinations where death has ensued from croup, *pure* and *simple*, I have failed to find satisfactory mention made of the discovery of a false membrane, such as I was led to believe formed in every case of croup.

I will later on speak of cases where we invariably have the exudation, and it is to establish to some extent a more correct differential diagnosis between the two conditions that I am induced to pen this article.

At this season of the year, in our northern climate, we are constantly meeting with cases of sporadic croup; or, in other words, an inflammation of the mucous membrane of the larynx and trachea. In the more severe cases we have considerable swelling caused by effusion into the sub-mucous areolar tissue, accompanied with secretion of stringy, often thick, tenacious mucus. The chink at the outlet of the larynx becomes smaller. The larynx and trachea are made narrower by the extremely congested state of the lining membrane. The same effusion into the sub-mucous tissue in other parts would produce no serious consequences; but occurring here, the danger becomes indeed imminent. Every moment, in fact, the case becomes more alarming; the breathing grows more and more difficult; respiration more and more diaphragmatic; the lips assume a bluish hue; the face grows pallid and anxious; the lungs become congested by an insufficient supply of oxygen; the dyspnœa, which has been permanent, now becomes spasmodic; the child grows very restless; no sooner has he been laid upon the bed than he begs to be taken up, immediately to be put down again; and thus the heart-rending disease progresses, until death by suffocation closes a painful scene, alas, too familiar to all of us. This suffocation, Steiner to the contrary, has not been produced by the formation of a false membrane, but simply by the inflammatory swelling and effusion into the sub-mucous areolar tissue. Nor do we find the inflammatory process confined to the larynx and trachea alone, but sometimes extending to the larger bronchi, though the tendency of this disease is rather upward, thus making the chances of relief more doubtful where treatment has been delayed.

A correct history of the attack should be carefully inquired into in every case, as this will greatly facilitate our diagnosis, and largely enable us to decide whether we have to deal with laryngeal croup or the diphtheritic form, in which the false membrane is always present.

We generally find that, a few days previous to the attack, the patient had taken cold, but very little was thought of it, as he was able to play around. There had been little or no lassitude and very slight fever. True, there has been a huskiness of the voice, and at

times a shrill, barking cough; but up to a few hours, perhaps, before being called in, there had appeared to the parents or friends no particular cause of alarm. But to the practiced ear there are the unmistakable symptoms of danger — the peculiar cough; the deep, stridulous breathing so pathognomonic of croup. High fever and extreme dyspnœa, and many or all of the symptoms before mentioned are possibly present. The case is evidently one of ordinary non-membranous croup, but in order to make our diagnosis clear beyond doubt, we make an examination of the throat and fauces, where we find no signs of exudation present. Thus the history of the case, along with close attention to the general physical lesions, and a careful examination of the throat, establish beyond question, no matter how severe the case may be, that we have present the catarrhal form, and that our remedies must be given to reduce the congested, swollen state of the lining membrane of the great air passage, and not for the purpose of removing a false membrane, as we once supposed necessary to cure our patient.

Now let us, as concisely as possible, point out the train of symptoms which present themselves in the membranous or diphtheritic form of croup. It may be asked: "Do the two diseases require a different treatment, so as to make a differential diagnosis of importance?" I answer, yes; it is of the greatest importance that we should settle in our mind at as early a moment as possible whether we are to prescribe for the catarrhal form or the diphtheritic, as the treatment, as I shall endeavor to show, widely differs.

When we have the exudation present, we will find a different set of symptoms from those mentioned in the laryngeal form. For some days, probably, before the medical man is called, the patient has complained of feeling tired; he has shown a disinclination to play. There has been considerable fever present, but *little* or *no* cough; and the cough, if present, is not, so far as I have observed, of the same husky, metallic character as in the catarrhal form. Later on the patient, if old enough to tell how he feels, will complain of stiffness and soreness of the neck and throat; he becomes more and more depressed, complains of feeling chilly, and "hugs the fire." He frequently puts his hand to his forehead, and says his head aches. Now the disease has fairly fastened itself upon the throat, the cough has become "croupy," and all the symptoms present in the non-membranous type are now developed. After carefully noting the history, we explore the mouth, uvula, tonsils, and pharynx, and find on some of these parts the characteristic exudation, formed to a greater or lesser degree. We are no longer in doubt as to the true nature of the disease. We have before us one of the most formidable maladies known to mankind; a malady that most of us would willingly resign the responsibility of treating; for we well know that, notwithstanding the wonderful success that usually attends the action of drugs when homœopathically prescribed, in diphtheritic croup, up to the present time, we are almost helpless to save our patient when the membrane has once fastened itself upon the larynx and trachea. I have treated over three hundred cases of the disease, and in spite of the best known methods of our art, the mortality has been large. We have here a disease clearly *per se*. It is not only dangerous from its position, which may rapidly produce death by suffocation, but we have to deal with an almost indestructible membrane, composed of numberless *bacteria*, which, on becoming re-absorbed, produce the alarming symptoms of blood poisoning. It is true, membranous croup rarely progresses so far as to permit of re-absorption, as it generally terminates life before this stage is reached.

Of course we will meet with cases where, from the advanced state of the disease when we are called in, and from the extreme irritability of the little sufferer, we are unable to explore the throat; and I am satisfied we have cases of diphtheritic croup where there is no false membrane visible to the eye. A few years ago I saw a beautiful child of five years die of croup where no exudation, from the most careful examination, could be discovered during life. But the case was beyond question diphtheritic, as established by the premonitory symptoms, and the fact that two of his brothers were ill with diphtheria when the little fellow was seized.

The absence of the false membrane on the tonsils or pharynx, together with the apparently sporadic character of the attack, often make the diagnosis more obscure between the laryngeal and diphtheritic types, but the obscurity is more apparent than real; for, as a general rule, we have the almost unmistakable history of the case to guide us. On the one hand, laryngeal croup is purely a local complaint, with little, if any, constitutional disturbance. The patient, possibly up to the moment of our visit, has been playing around the room. True, we may have the same characteristic expectoration as in pseudo-membranous angina, but there is the spasmodic dyspnœa, with increasing paroxysms of croup, the husky voice, and the tendency of the disease to spread upward. On the other hand, in membranous croup we have the premonitory symptoms — the shivering; the general malaise and fever before the croup commenced; the usual soreness and, perhaps, swelling of the throat; and, at the time of our visit, the more or less marked constitutional symptoms.

Catarrhal laryngitis, or ordinary croup, is dangerous only from its position. Diphtheritic or membranous croup is dangerous both from its position and the tendency of the exudation to spread downward, and the liability of re-absorption taking place, even when we have apparently relieved the croupal symptoms.

Treatment.—Delays are generally dangerous, but in either form of croup they may prove fatal. As we are usually admonished by our professors of midwifery to "Go with obstetric haste," in all cases of confinement, I would give the same advice in croup. See your patient at the earliest moment possible.

In the laryngeal form the first thing to be done is to place your patient in the best position for recovery. Have him undressed and put to bed between blankets in a flannel night dress. Rest is a very necessary factor to the case. Then see to the temperature of the room; have all drafts closed, a brisk fire made in the stove or grate, and as rapidly as possible raise the temperature to 75 to 80 degrees Fahr. Have the teakettle placed upon the fire with sufficient water to allow the steam to pour out copiously from the spout when boiling. An india rubber flexible tube carefully fitted over the nozzle of the spout and brought in careful proximity to the sufferer's mouth, greatly increases the usefulness of this steaming process. But on no account should any medicinal substance be added to the boiling water; a pure, moist atmosphere is the object desired. The soft vapor acting upon the swollen mucous surface is a very important agent in the reduction of the same.

I prefer hot fomentations to the cold pack; I consider them safer, and of late years have invariably employed them. I am aware that many will not agree with me on this point. Steiner, in Ziemssen's Cyclopædia, strongly recommends cold in the "form of frequently changed cold compresses about the neck," and even speaks favorably of the cold baths advised by Bartels; and, while I do not deny the advantages that may at times accrue from this antiphlogistic plan, in my own practice I have had more satisfactory results from the employment of flannels or sponges wrung out in as hot water as the patient can bear. So far as the internal treatment is concerned, unhappily I must admit I am able to throw little, if any, new light upon its therapeutics. In the inflammatory stage, or better, in the stage of invasion, we have no remedy that compares with *Aconite*. The potency must be a matter of

personal experience. I prefer the low attenuations, and often in urgent cases prescribe the crude drug. In *Aconite* we find our most powerful weapon throughout the treatment of laryngeal croup. Next in order I have found the best results from *Spongia*, *Bryonia*, and *Phosphorus*, administered at the first decimal potency every fifteen minutes, or even oftener in extreme cases. In the use of *Tartar emetic* I have found little or no service. In the higher triturations it has seemed inert, and in the lower I have noticed it produce a marked diminution in the patient's strength. Before speaking of what may be considered as a last resort—tracheotomy—I would call attention to a drug which has been employed empirically by both schools of practice in England, namely: *Chloral hydrate*. My experience with the drug in croup has been very slight, but Dr. John Barclay mentions several cases where all chance of recovery seemed removed, that were saved by the administration of *Chloral*. "Before the *Chloral* was given," he observes, "the respirations were from 60 to 70 per minute, the pulse varying from 144 to 156, and weak; the dyspnœa was extreme, and the voice and cough now close and dry. Within two hours after the first dose, great relief in the breathing was observed, the child slept half an hour at a time, the respiration and the pulse fell a little, and, what was more curious, a very copious exudation of mucus began in the larynx and trachea. Indeed, so great did this become that it was necessary to raise the child every half hour, when a little water or sherry was given to produce a cough, by which the air passages were cleared of the mucus. * * * * Gradually the pulse fell and the respirations became slower, till at the end of twenty-four hours the former was only 108 and the latter 30. This treatment was persevered in for forty eight hours, after which milk and sherry only were given. The recovery was slow but steady."

The dose advised is one grain for each year of the child's age, and administered every six hours. In apparently hopeless cases it is certainly worthy a trial, more especially since the present state of our strictly homœopathic therapeutics in croup is far from encouraging. Now, with regard to tracheotomy, the profession seems divided upon its merits. In nine operations under my notice it failed to save life, but was instrumental in giving ease to the little sufferers.

If it is to be performed, it should not be deferred until the patient is poisoned with carbonic acid, and I believe, where it is resorted to early enough, more uniform success is obtained. Steiner says the time to operate is in what he calls the beginning of the third stage, the so-called stage of asphyxia; but it seems to me that there is considerable risk in waiting until this stage is reached, as the rapidly congesting lungs may cut off all chances of success. I prefer to operate before so extreme a condition is present. But the great difficulty in the way of tracheotomy is the opposition of parents. I have many a time seen some bright boy or girl suffocated when I believe a timely opening of the windpipe might have saved his or her life; but the mother, or father, or both, would sooner let their child die than try so harsh a resort.

The management of diphtheritic croup differs from laryngeal, inasmuch as we have to treat a constitutional disease, of which the false membrane is only one phase. Our treatment must be essentially the same as ordinary diphtheria. The croup is produced by the presence of a foreign body in larynx or trachea, and its removal can only be effected by a treatment that will act specifically upon the disease itself—and here, unfortunately, we must confess our helplessness. Some years ago I flattered myself that, with the *Biniodide of mercury* and *Permang. potass.*—the one internally, the other applied to the throat locally—I could cure most any case of diphtheria or diphtheritic croup that might come under my care, but a more extensive experience has since convinced me of the folly of my confidence. The literature of the treatment of diphtheria is wonderfully voluminous, and as wonderfully useless when put to the test; and we feel surprised that scientific men (and all medical men should possess a scientific education) have been rash enough to rush into print to advocate remedies that are in most cases worse than useless, as precious time is lost in trying them. Notwithstanding the want of success that has often followed the administration of the *Biniod. of mercury* in my hands, I am satisfied that, up to the present time, it is the best general internal remedy we have.

The next most important remedies we have are *Alcohol* and diluted *Chlorine water*. From the experiments of Oertel, these two substances produce the most marked effect upon the micrococci. I have found that port wine, either alone or combined with the *Chlorine*, is the best form to administer *Alcohol;* and it is a matter of surprise often the large quantity of this wine that may be consumed by a diphtheritic patient without producing any sign of intoxication.

I notice that some of our writers tell of the wonderful results obtained from *Lac. can.* 100,000; even one dose curing a most "frightful case!" Others speak of *Bell.*, 100,000, and *Sac. lac.*, in a single night relieving a most alarming attack. I gave two years ago the so-called *Lac. caninum*, C. M. and 1. M., whatever these mean, a fair trial in several of my worst cases, but I failed to obtain the slightest satisfactory result; and I obtained, through a friend, the medicine and full directions from Dr. Swan himself. So far as the administration of the *Kali permang.* is concerned, I have of late years come to doubt its efficacy in the treatment of diphtheria or diphtheritic croup. I have given the hypodermic injection of the *Kal. permang.*, as advised by Dr. Okie, in two hopeless cases without any effect. The chief value of this remedy is that it removes the very disagreeable fetor in malignant cases.

A specific remedy for diphtheria has yet to be discovered, and the physician who shall discover it will be considered one of mankind's greatest benefactors.

ON THE TREATMENT OF MORBUS COXARIUS BY A NEW METHOD.—1st, to secure immobility of the joint; 2d, to procure extension of the limb; 3d, to take off from it the superincumbent weight of the body; 4th, to provide means to enable the patient to take exercise in the open air. To obtain extension of the limb and to remove the weight of the body, he resorts to the following device: On the shoe of the sound limb an iron sole is applied, three inches high, so as to raise the foot from the ground. This elevated shoe and a pair of crutches constitute the apparatus. As the patient stands on his crutches the diseased limb is suspended. The shoe should be high enough to prevent the toes of the affected limb from touching the ground.

Dr. JOSEPH C. HUTCHINSON.

I HAVE reduced gradually to normal frequency (and thus saved the patient) a pulse, in puerperal fever, that ran 150 beats per minute, by half-drop doses Norwood's tincture Veratrum viride, with one grain quinine administered every two, three, and four hours. I recently examined several patients in Brigham Hall Asylum by "cranial percussion," with a view to "cerebral localization." In a respectable percentage I succeeded. In one epileptic lady, whose skull was broken many years ago, I was enabled to point out distinctly the seat of fracture.—DR. R. J. HUTTON, *in Medical and Surgical Reporter.*

CHLORAL HYDRATE, in a 50 per cent. solution, is claimed to be of service topically in diphtheria, reducing the strength of the solution as granulations are observed.

CLINIQUE.

OVARIOTOMY.*

A SUCCESSFUL CASE IN WHICH THERE WAS NO PEDICLE.

By JOHN C. MINOR, M. D.

Mrs. R., of Waterbury, Ct., 59 years old, married, primaparous, had carried the tumor for five or six years. The tumor was a polycyst of the left ovary, and had been tapped three times, about forty-five pounds of fluid being removed at each tapping. Several years before, she had been troubled with uterine hemorrhage produced by polypi. One of these was removed, and the hemorrhage had gradually ceased. Condition at the time of operation excellent. She was thin, sinewy, courageous; digestion in fair order; skin and kidneys in perfect action.

Operation, Oct. 7, 1879. Present: Dr. George P. Swift, who was also the attending physician; Drs. Osborn and Gregory, and Mr. Wm. Bryan, medical student. The usual preparations having been made, an incision five and a half inches long laid open the abdominal cavity and exposed the tumor. A sound passed between the tumor and the abdominal wall detected firm parietal adhesions on the right side, comparatively few on the left; and deep down in the pelvis there seemed to be either extensive pelvic adhesions or else an unusually broad base—a point that could not be determined at this stage of the operation.

A large Hutchinson's trocar was then thrust into the tumor, but the pressure from within split up the opening made by the instrument, and the fluid gushed out in a stream that was beyond control. By inserting my fingers into the tumor, and holding the lips of the rent in firm contact with the sides of the incision, while my assistants turned the patient so far over that everything must run out and nothing could run in, not a drop of the fluid entered the peritoneal cavity.

When the tumor was thus emptied of its most fluid contents, I attempted to draw it outside of the abdomen, but found this to be impossible. I detached the adhesions on the left side, without any effect in moving the tumor. Attacking the adhesions on the right side, I found them very dense in the neighborhood of the former tappings, so that they had to be divided with the knife or scissors. Other adhesions, of less extent and firmness, were stripped off from the tumor until it seemed that sufficient detachment had been made to enable me to lift the base of the tumor to the external wound. It was still impossible, and so I passed my hand into the tumor and broke up a large collection of secondary cysts containing colloid matter, in order to so diminish the size of the tumor as to enable me to get at its base. In this procedure, however, I was unfortunate enough, in attempting to rupture a cyst, to make an opening, through which colloid matter, to the extent of over a pint, escaped into the peritoneal cavity. A careful examination made it evident that the trouble was at the base of the tumor, and much more serious than I had anticipated. Passing my hand in the peritoneal cavity down to the base of the tumor, I found it had no pedicle, properly speaking. It seemed to spring from nearly the whole of the broad ligament on both sides, and in the centre of this broad base could be felt a hard fibrous body, that was probably the uterus. Then, inserting my hand inside the tumor down to the very bottom, I felt the projection of a hard body, the shape and size of the fundus uteri, rising into the tumor about an inch and a half. While one hand grasped this body, I inserted the other into the vagina, and, by conjoined manipulation, demonstrated the fact that the uterus was between them. The case was now tolerably clear. There was no pedicle, for a ligature applied at the lowest point of the base of the tumor would encircle the middle of the uterus. The tumor developing in the left ovary had become adherent to the broad ligament, had encroached upon, and finally enveloped the fundus of the uterus, had spread over the broad ligament of the other side, and incorporated the other ovary in its growth.

The method of enucleation was the only way by which the tumor could be removed, and it was put in practice at once. Beginning as low down toward the base as I conveniently could, determining the point somewhat by the size of the vessels contained in the cyst-wall (about four inches above the base of the tumor), I carefully dissected up an edge of the peritoneal investment all around the tumor. Then I carried the process of enucleation down on one side of the tumor to its base, over the uterus and broad ligaments, and up on the other side until the whole mass was detached and removed. During the process of enucleation there was considerable hemorrhage, and the blood escaped freely into the peritoneal cavity, but there were no arteries of any size, and I could find no occasion for more than four or five silk ligatures which were applied upon such of the adhesions as seemed to possess a dangerous amount of vascularity.

The cavity of the peritoneum was then cleansed with soft sponges, wrung out in carbolized water, and this process was done with more than usual care, because the amount of colloid matter and blood that had escaped during the operation seemed to insure the occurrence of septicæmia. I am positive that a considerable quantity of blood and colloid matter must have escaped the cleansing process, and remained in the peritoneal cavity, in spite of the time and care I bestowed upon it. In fact I have yet to learn a method that will remove colloid matter thoroughly and safely when it has been poured over the intestines. The enucleated cyst-wall had stopped all oozing of blood, but it did not seem to be a very promising portion to retain in the abdomen, for its inner surface was covered with coagulated blood, and at its base was the uterus, apparently denuded of peritoneum and covered with coagula. The appearance was really worse than the fact, for I had removed all coagula that could be safely detached from these surfaces.

There was nothing else to do, however, and so I inserted a large glass drainage tube, not into the enucleated cyst-wall, but behind it, in Douglass' cul-de-sac, with its external orifice at the lower angle of the wound. All the ligatures were cut short, and the wound closed with silk sutures in the usual manner.

The estimated weight of the tumor was between fifty and sixty pounds. The cyst, emptied of its contents, weighed five and a quarter pounds.

The after-treatment of this case, which was conducted to a successful issue by the skill and judgment of Dr. Swift, was in some respects peculiar. No morphine was given, nor anything to confine the bowels. She was allowed to urinate, without the introduction of a catheter, from the second day on. The tube was retained three weeks; the stitches for fourteen days. This long retention of the tube was due to the fact that we apprehended septic fever as a necessary sequence of the escape of foreign matter into the peritoneal cavity, but the temperature never rose above 102, and her progress toward recovery was uninterrupted. Chloroform was used, but there was no nausea afterward, although the operation occupied over two hours.

* Read before the N. Y. Medico-Chirurgical Soc., Nov., 1879.

BURNS.—Experience is claimed to have shown that the *Bi*-carbonate must be employed in saturated solution, ice-cold. Any deviation from the above is responsible for failure.

CASES FROM PRACTICE.

By G. Buckingham Smith, M.D., New York.

Mrs. J. C. B., aged 58, tall, slender, light eyes and hair, skin dry and wrinkled, had suffered from constipation of the most aggravated form for about twelve (12) years. The bowels were not moved oftener than once in two weeks or a month without the use of violent cathartics or enema. Appetite small, but taste of food normal. No bloating of abdomen or any other unpleasant symptoms, except terrible back-ache, with protruding hemorrhoids of a dark purplish color, without much bleeding. Had passed her climacteric without any unpleasant symptoms.

Saw the case first on the 19th of July; gave one dose, in office, of *Plumbum m.*, 3d, to be followed by *Æsculus h.*, 1st, in alternation with *Collinsonia c.*, 1st, two powders of each per day, taken as follows: *Æsculus h.* before breakfast and dinner, and *Collinsonia c.* before lunch and at bed-time; medicine enough to last a week.

Monday, July 28, reported very much improved; bowels have moved every day since the third day after beginning treatment.

Continued same medicines, with the addition of three powders of *Sulphur*, 3d, before beginning on the *Æsculus* and *Collinsonia;* medicine to last a week to be taken as before. Reported at the end of the week bowels moving every day, hemorrhoids almost entirely disappeared, *no* pain in the back, and a desire for larger quantities of food than she had taken before. Continued same treatment, except that only two doses of medicine were given each day — *Æsculus* before breakfast, and *Collinsonia* before retiring; medicine to last two weeks, as the patient was to leave for her home in the northern part of the State. No further medication. Had report of case about the middle of October. Bowels moving every day; no pain in back; hemorrhoids disappeared; appetite good; don't care to take any more medicine, as she considers herself cured.

Mrs. H., aged 36, blonde, large, full blue eyes, and brown hair, skin extremely white, in perfect general health, weight about 163 pounds at the time I first saw her, had suffered from gall stones from about her 30th year. Had been treated by a number of allopathic physicians for neuralgia of stomach, neuralgia of intercostal muscles, etc., etc., without results, except to disgust her with the whole fraternity of doctors. At last some one suggested that she might have gallstones, and on that suggestion she underwent the usual course of iodides, bromides, nitro-muriatic acid, etc., etc., besides drinking quarts of olive oil, without any improvement of her condition except as to flesh. The oil made fat, and as she gained in adipose, her difficulty increased. As there was no improvement in her condition, she gave up treatment, and made up her mind that some day a stone too large to pass would get into the duct, lodge there and cause her death.

I was called to her in May, 1876, and found her suffering from the passage of a stone. I immediately gave *China*, 3d, which relieved her sufferings materially, as she said, "That was a small one, or else your medicine helped me." As I had faith *not* larger than a grain of mustard seed, I said very little about the action of the drug, but told her all I knew of its action, and proposed to put her under treatment continuously and see what would be the result.

Up to this time she had been having a recurrence of pain and passage of stone sometimes as often as every day, and never being free from a recurrence more than a week or ten days at the longest, the attacks sometimes lasting three days before relief was obtained by the passage of the stone. I put her on *China*, 3d, a dose three or four times a day. The dose was one lozenge properly saturated. This treatment was continued for at least six months, and during that time the passage of the stones became less frequent and painful, until they ceased altogether. She has taken no medicine for over two years, neither has she had any of the old trouble.

I think it fair to pronounce the case cured, as there have been no signs of the disease for twenty-four months or more.

RETENTION OF URINE.

By J. C. Shaw, M. D., Eagle Bridge, N. Y.

Mr. H., age fifty years, received a fracture of the femur, lower third. After reduction of the bone there was retention of urine, for which the usual remedies were given; hot fomentations applied, too; of no avail. The difficulty was not overcome until the introduction of ice into the rectum, about the size of an acorn, and form of an elongated oval, which I passed beyond the sphincters. In ten minutes, urethral spasm ceased and urine passed freely.

A Case from Charity Hospital, Blackwell's Island.—Three weeks ago Mrs. Sheridan, who resided at Fifth avenue and 146th street, was, as she alleged, kicked in the abdomen by her husband. She was removed to the Charity Hospital on Blackwell's Island, where Dr. Eastbrook had her cared for. Last Friday the Coroner's office was notified that she was dying, and, upon the strength of her ante-mortem statement, her husband was arrested and confined in the Tombs. Finding that it was impossible to save her life, the medical staff concluded to perform an operation by which the woman would be delivered of her unborn babe. This operation took place, and the child was brought into the world by the "Cæsarian operation." Shortly after the operation the woman died. Her body was removed to the Morgue on Tuesday, and accompanying the remains was a death certificate, which stated that the cause of death was puerperal fever. Nothing, however, was said regarding the fate of the child, and Dr. Cushman could not learn whether it was born alive or dead. Yesterday morning he made an autopsy of the body with the most astonishing result. He discovered another unborn baby, which evidently had been overlooked when the doctors relieved the mother of the first child. Baby No. 2 was well formed and large, and when alive must have been healthy. There was no appearance or indication of puerperal fever, but instead there was a malignant form of Bright's disease. There was no evidence of violence, externally or internally. Dr. Cushman therefore believes that the woman made a false statement when she charged her husband with having kicked her.—*Herald.*

Obstetric Forceps.—Dr. A. H. Smith, of Philadelphia, takes the position that, for the safety of the mother and the child, the obstetric forceps should be kept in median line, and should not receive any lateral or pendulum motion. Dr. Smith regards the leverage power of the forceps as a most dangerous delusion, and that as an aid to the traction efforts of the forceps it had no foundation in fact.—*Obst. Gazette.*

To Prevent Boils.—A very simple remedy is made known by Dr. Seiren, in a St. Petersburg journal, for preventing the development of boils. He states that if the skin is superficially scraped with a small knife, so that a drop or two of blood may be pressed through the epidermis as soon as the peculiar stabbing or pricking sensation and slight induration announce the commencement of the boil, it will not be further developed.

The Homœopathic Times.

A MONTHLY JOURNAL
Of Medicine, Surgery and Collateral Sciences.

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Published on the First of each month.

Office, 18 West Twenty-third Street, New York.

NEW YORK, DECEMBER, 1879.

"*A regular medical education furnishes the only presumptive evidence of professional abilities and acquirements, and OUGHT to be the* ONLY ACKNOWLEDGED RIGHT *of an individual to the exercise and honors of his profession.*"—Code of Medical Ethics, Amer. Med. Ass., Art. iv., Sec. 1.

RETROSPECT.

The time rapidly approaches when we must decide upon our course in respect to the "Retrospect of Literature."

The question is, whether we shall continue to publish it?

We should be glad to know the wishes of our subscribers in this regard.

MEDICAL AND SCIENTIFIC TESTIMONY.

A medical man, or a specialist in any department of physical science, may be placed upon the stand as an ordinary witness, in which case he is subject to the rule governing all witnesses, and is required to state simply what he knows, what he has seen or heard, with no more latitude of opinion than any other witness. When, however, he is called upon as an expert, the case is different. Here he is expected to give opinions and the reasons for his conclusions. Of course the value of the evidence depends, in a great measure, upon the honesty and intelligence of the witness. When we take into consideration the fact that ordinary juries are generally men of no particular scientific attainment, we can easily understand how an expert, fully conversant with his subject, with a theory in his mind—perhaps right, and perhaps wrong—could impress his own convictions on their minds. They are dazzled with scientific terms of which they are ignorant, and by evidence the full force of which they cannot appreciate. In more than one case, where the question of insanity is at issue, and where the lines are so sharply drawn that life hangs upon conclusions drawn from minute points, there is every reason to believe the scale has been turned to the death side by theoretical reasoning of so-called experts; and in more than one case, also, the guilty has escaped punishment because a feed expert had a theory of his own, not always founded, however, on sound scientific deductions from facts. In the murder case in Connecticut, which has for so many weeks dragged its weary length along, every one has been struck with the bewildering testimony of scientific experts, and the conclusion is strong in many minds that, if the prisoner escapes conviction, it will be through the scientific fog raised by the prosecution. Jurors are only human, and we must expect, when we see such an avalanche of scientific detail rolled upon them, that, at the end of the trial, they will either be in an advanced condition of dementia, or in a high state of acute mania. No member of the legal profession can be expected to master all the minute details of every scientific specialty; and when he pits one expert against another, as he is often obliged to do, it will require all his legal skill, and all the wisdom of the court, to prevent the minds of the jury from becoming confused. The only safety for a medical expert is, before he advances an opinion, to study his case carefully, and, divesting his mind as much as possible, confine himself to legitimate deductions from present scientific facts.

STATE INSANE ASYLUMS.

The great public is in a periodical state of grumbling at its public servants. This, in one sense, is a healthy condition of society; for, when a public man knows that some time or other he will have to pass under fire, he is pretty sure to be always ready for a thorough investigation. Just now the public press is hurling its thunders at the State Insane Asylums, and calling for a thorough legislative investigation of these institutions. If any public institution in the State is hedged round with stronger safeguards or more judiciously managed than its asylums for the insane, we have to hear of them. The superintendents, without exception, are gentlemen of culture, refinement, and in point of scientific attainments will compare well with the heads of any medical institutions in the world. Errors of all kinds are carefully guarded against, and the workings of the institutions are as perfect as competent medical officers and zealous, watchful trustees can make them. In every ward of the asylum letter-boxes are placed, which can only be opened by the Medical Committee of the Board of Trustees. Into these boxes patients can put any statement they choose, and we can answer for Middletown, that four times a year these boxes are opened by the proper authority, and every paper carefully noted. The medical officers form their own opinions of the sanity of their patients, and any attempt at imposition will sooner or later come to grief. We assure the public that the State has already taken every necessary step for the proper

care and protection of its insane, and all that careful nursing and enlightened skill can do to restore those intrusted to their charge to the light of reason and to their homes will be done.

DR. ALLEN'S LAKE GEORGE TEST.

To the Editors of the Homœopathic Times:

Will you kindly publish in your next issue the fact that, when I applied to Dr. Wilson for a committee, I was not aware that Dr. Henry D. Paine had been appointed by Dr. Dake. Immediately on learning this, I notified Dr. Wilson, and apologised to Dr. Dake.

I am now working with Dr. Paine, and, if health and time permit, will report next June. T. F. ALLEN.

(The card of Dr. T. F. Allen we publish with pleasure. We presume, however, our readers will be at a loss, like ourselves, to see how the appointment of Dr. Paine, by request of Dr. Allen, as the custodian of bottles and records, can account for the refusal of Dr. Allen to go on with his experiments, under the auspices of the Bureau of Materia Medica, Pharmacy and Provings.

We are informed by the Chairman of that Bureau that, while he consented to Dr. Allen's selection of Dr. Paine, it was distinctly understood that the safeguards considered important and suggested by Dr. Sherman, should be applied in the preparation and marking of the bottles, so as to render the conditions of the test unquestionable.

And, as mentioned in our last issue, Dr. Allen, after his resort to Dr. Wilson, gave up the whole matter into the hands of Dr. Dake, and nominated Professor O'Connor as his committeeman, leaving out entirely the arrangements with Dr. Paine; and yet he now says, "I am working with Dr. Paine."

We confess to some difficulty in comprehending the motives of Dr. Allen in "tacking about" so much, in the performance of a work proposed by himself and aided by the Chairman of the Bureau to which all such undertakings belong, so far as the American Institute is concerned.

It would seem that, if Dr. Allen really intended to furnish a convincing proof of the power of the thirtieth centesimal dilution or "potency" of a drug, he would have gladly accepted Dr. Sherman's safeguards and the supervision of the appropriate Bureau.

As it is, we fear his test, however successful he may be in picking out the medicated bottle, will have little weight upon the minds of persons who favor exact methods. What better can it be than the numerous "provings" of that attenuation, by persons who choose their own methods and conditions, regardless of the scepticism of the medical world around them and the demands of science.

So far as we, personally, were concerned, we consented to act on the committee, at the call of Dr. Dake, believing that we could thus aid in the settlement of a long mooted and vexatious question.

As a member of the Bureau of Materia Medica, Dr. Allen may submit the results obtained from his work with Dr. Paine, and they will be taken for what they are worth by the Institute and the profession.

We confess to a want of confidence in the methods generally pursued in our school, in determining questions like this, as well as in building up a materia medica upon the reliability of which thousands of lives must depend.—EDS.)

N. Y. OPHTHALMIC HOSPITAL.—Month ending October 31, '79: Prescriptions, 3,793; new patients, 442; resident patients, 33; average daily attendance, 140; largest attendance, 200.

J. H. BUFFUM, M.D., *Resident Surgeon.*

SPIRITUALISM vs. HOMŒOPATHY.

A Kind, Parting Word to "E. N. E. of Baltimore."

BY AN INQUIRER.

With many thanks for your effort to answer my inquiries, allow me to say that I am not in the ring as your antagonist, nor is it any part of my purpose to enter here upon the grave questions so flippantly stated, and so cavalierly treated by you in the September issue of the TIMES. Your free use of the terms, "materialists," "sceptics," "pearls before swine," "capable only of eating husks," and others of like character, applied to those who do not accept the theory of *endless dynamization*, and the whole spiritual, medical philosophy advocated by yourself and a small part of the believers in homœopathy, led me to propound, for your consideration, a few questions in the October TIMES. I entered upon no argument whatever—my interrogatories were simply to elicit the truth from your own pen, and to secure for the great body of our profession merited justice at your hands, so far as you could be induced to exercise it.

Seeing that you were a writer of more than ordinary ability, I felt that you could appreciate the weight of facts, as well as lightness of fancies, in dealing with subjects of great practical interest; and that you would drop the exuberant pharisaism and denunciatory style of your first effort, upon a sober second thought.

In a measure you have met my expectations. You have answered some of my questions very fairly, while some have been passed by without an attempt at an answer of any kind. You realize how hard it is to answer interrogatories calculated to develop the weakness or absurdity of one's own arguments.

You seem to think I did not understand your *animus*. If I was at all in doubt about that, after reading your first article, I surely am not now. But I desist.

As to myself, you say I am "an extremist." I suppose it will be as well to let the readers of my questions, addressed to you, judge of that. I am not especially concerned about it.

But I must here say that the question of dose and drug preparation is just as legitimate and proper, yea, even as necessary, as any other suggested by the law of similars; and there is no more occasion for the use of hard epithets, and of intemperate zeal, and poor logic, and nonsense, in its discussion, than in the discussion of any other question.

The attempt to ostracise and cast out, as "non-homœopaths," "anti-homœopaths," "outsiders," "eclectics," "materialists," "infidels," all who do not believe in the theory of *infinite drug potentization*, is the most ridiculous farce on record. And the timid outcry of many, so intent on gathering in shekels, or compiling books, or carrying on other schemes for which a *universal patronage* is desired, lamenting the discussion of the potency question, and calling for a perpetual truce, would be the most *comic* farce, if not so seriously and mournfully stale.

It is the individual right of each qualified practitioner to determine the *quality* and *quantity* of the remedy, in each case under his treatment—not only his right, but likewise his *duty*; and he cannot well discharge that duty without a dispassionate consideration of all that is known on the subject.

What is wanted is a scholarly and dignified discussion of the potency question, without personalities and without bitterness. Let facts be stated and sifted t ll principles are arrived at, at once in keeping with the demands of science and the necessities of practice.

In conclusion, allow me to say that I am your friend and brother, with many years of experience in the use of attenuated doses; never yet convinced of the necessity of "high potencies," yet willing to look all posological facts squarely in the face, and ready to accept the truth, whatever old opinions and practices may thereby be cast aside forever. AN INQUIRER.

A SUIT FOR MALPRACTICE—ELDRIDGE vs. FLAGG.

MESSRS. EDITORS: In view of its common interest to the profession, and because the daily papers have already published the result without giving the honest facts, I venture to give in detail the whole story of the suit for malpractice which was tried at White Plains, N. Y., on the 22d of September, 1879, before Judge J. O. Dykeman, in which the plaintiff sued for $5,000, and was awarded $100. I do this with the full consent of the defendant, Dr. L. W. Flagg. The case is of peculiar and personal interest to me, as the sequel will show.

The plaintiff, Mr. Harry C. Eldridge, is a young man, having been married about two years, and is a street or "curbstone" broker in Wall Street. He came, with his young wife, in the autumn of 1878, to the Mansion House at Yonkers, where he was during Dr. Flagg's treatment of him.

I need not pause to tell who Dr. Flagg is. His prolonged and successful career of more than thirty years as a homœopathic physician in Yonkers has made his name familiar to the profession, especially to the older members.

Early in February, 1879, Mr. Eldridge came to Dr. Flagg's office to be treated for gonorrhœa. He denied having had illicit intercourse, and as his wife is above suspicion, it was decided that it was caused by his wife's leucorrhœal discharges, and by his having had connection with her during her menstrual periods. He admitted that he was amorous, and that he had indulged in sexual excesses to that extent that his system was very much reduced. And so it was. He was simply "used up." Dr. Flagg treated him at his office for about two weeks, when, upon his own responsibility, he ceased to present himself. Ten days passed, Dr. Flagg hearing nothing from him, till on March 3d he was sent for to visit him at his room at the Mansion House. Here he found him in bed. Upon examination it was found that he was suffering with general rheumatism, which was, no doubt, a complication of his urethral inflammation, and was gonorrhœal rheumatism. He saw him daily for three days, and then omitted one day. During the whole time he improved rapidly, so that on the third day he became uneasy in bed, and desired to sit up. This Dr. Flagg permitted him to do, and *playfully remarked that there was no danger of a rheumatic patient taking too much exercise*, meaning, of course, that the pain on motion would force him to be quiet. He complained more particularly of his right knee, and Dr. Flagg says he examined it, and found it slightly reddened and swollen. In addition to his remedies (*Aconite, Bryonia, Rhus*, and *Salicylic acid* as indicated), he directed the knee to be wrapped with flannels wrung out of hot water.

On Friday (or Saturday), previous to his departure on Saturday for Mexico, he called somewhat in a hurry, and found him sitting up (or reclining), and feeling comfortable. At this visit he did not strip his knee and examine it, because he did not complain particularly of it, and because all fever had left him, and he seemed to be doing handsomely. At this visit he informed him that he was going to Mexico to be absent one month; that Drs. Howe and Phillips would have the care of his practice, and that, if he so elected, Dr. Phillips would take charge of him, to which he fully assented.

He was so much improved that it was so arranged that I should not call until Monday, unless I should be sent for, which was to happen in case of any new developments. On Monday I called. He was sitting in his easy chair, and was comfortable mentally as well as physically. Upon examination I found the knee without *heat* or *redness*, but with slight puffiness on either side of the patella, and with fluctuation. I informed him that his condition had changed; that there had been effusion, and that he now had synovitis sub-acute, or hydrarthrosis. I instructed him to keep his knee perfectly quiet. He remarked that Dr. Flagg had told him he might take exercise, to which I replied that, "If Dr. Flagg should see the knee now, he would not tell you to take exercise." I also told him that this new condition was a sequel or result of the rheumatic inflammation. He further remarked that Dr. Flagg did not examine his knee. I gave him *Bryonia*, and continued the flannel wrapping. On Thursday I called again, and found him still comfortable, and certainly no worse. At this visit I examined his urethra, and found the gonorrhœa still present. I ordered them to get an elastic knee-cap, to be used as soon as he could bear it, and also directed the knee to be painted with the tincture of iodine. I called again the next day, Friday, and applied a roller bandage, beginning at the foot, using great care in its application to the knee, lest it should be too tight and painful; also told his wife to continue the iodine, and reapply the bandage *with great care*; and that, if there was any pain in the part, she must send for me; and that it was possible that I should have to draw off the water with a needle. The *Bryonia* was continued, and I was to call again on Monday, if not sent for before.

On Sunday I received a note which read as follows: "Dear Doctor: I am sorry to say that we have decided to change our mode of treatment. Yours, etc., Eldridge." On Monday, as I was visiting another patient at the same (Mansion) House, I dropped in to inquire why I had been thus summarily dismissed. I found the patient in bed, and he freely told the following story: "The bandage had given him pain; the landlord, Mr. Shears, had visited him, and had told him he had a bad knee; that Dr. Phillips was a young man, a new comer, and a homœopath; that he had better send for Dr. Upham to come and examine his knee, as he (Dr. Upham) had been his (Shears') family physician for twenty years, and was a man of experience, and an allopath, and especially because Eldridge had always before been treated allopathically. Accordingly Dr. Upham was summoned to at least have a look at the case. Dr. Upham told him that what Dr. Phillips was doing was well enough, but it was too mild; that he had just had a case exactly the same, in which he had drawn off the water with blisters, and that the case was now well." Eldridge said, "If what you say be true, then, of course, I want you to take the case." The knee was at once blistered, and the note of dismissal was sent to me. Dr. Upham had just been there, had pricked the blister, had drawn off the water (?), and had ordered a fresh blister on the same spot, which covered the entire knee.

Mr. Eldridge seemed satisfied that that was the true way to get rid of the water, and with much delight and satisfaction asked me to look at his knee, which was completely skinned. After two or three days' attendance, Dr. Upham, Sr., transferred the case to his son, who, he said, was more of a surgeon than he was, and who had finished his studies at Bellevue Hospital only a few months previously. Dr. Upham, Jr., it seems, adopted the opposite treatment, and applied ice bags. He kept the case under treatment for about two months, during which time the other knee was attacked.

After Dr. Flagg's return from Mexico, he called on Mr. Eldridge, and presented his bill. About a month or so later he received a note from Mr. Eldridge's attorneys, Burrill, Davidson & Burrill, of New York city, stating that Mr. Eldridge claimed damages, and that if he would call and settle, he would save expense. Dr. Flagg considered it an attempt at blackmail, and so treated it with indifference. A month or so later the complaint was served. The plaintiff complained of carelessness and neglect on the part of Dr. Flagg, because of which he had suffered great pain, and had

been kept away from his business, which had been greatly damaged. He asked for $5,000

The venue was laid in New York, and was changed by Dr. Flagg's counsel to White Plains. For a month or two all was quiet. Then one day Dr. Upham, Sr., called on Dr. Flagg, and in behalf of his son, said that he had heard of the affair about ten minutes before, and had hastened to explain. He expressed his disgust with the affair, and assured Dr. Flagg that Mr Eldridge's conduct had not been influenced by anything they (the Uphams) had done, and that he might count on their support in the trial. He also said that he did not know of the existence of gonorrhœa until two weeks after he had given the case to his son.

Finally the time for the trial came, and a fruitless attempt was made by plaintiff's counsel to have the venue changed back to New York city. At this juncture, Burrill, Davidson & Burrill seemed to get ashamed of themselves, and transferred the case to Mr. M. J. Keogh, a young criminal lawyer of New Rochelle, he being assisted by some of the juniors from the firm of B. D. & B.

The Judge appointed Friday for the hearing of the case. The plaintiff appeared with his medical experts, Dr. Stephen De Wolf and Dr. Stuyvesant F. Morris, both of New York city, and with brokers to testify how much his business had been damaged. His only witness as to the facts in the case was Dr. Upham, Jr., who, it will be remembered, took the case from his father, who took it from me (and blistered it), who took it from Dr. Flagg, so that he did not see it till ten days after Dr. Flagg had sailed. An unfinished case lasted all day Friday, and on Monday all hands were again present to find the case still on, the medical experts being re-inforced by Dr. Wm. T. Bull, resident surgeon at the Chambers Street Hospital.

Dr. Flagg had made arrangements with his friend, Dr. Wm. Todd Helmuth, to come up when he should be telegraphed for. Unlike plaintiff's experts, Dr. Helmuth's business would not permit of his hanging around two whole days for a chance to distinguish himself.

At two P. M. on Monday the case was called, and Dr. Helmuth was at once sent for, but unfortunately he had decided that at that late hour he would not be sent for, and had gone out of town to operate.

The plaintiff swore that he had suffered from synovitis, and had called in Dr. Flagg, who, without examination of his knee, of which he had especially complained, had diagnosed his case rheumatism, and had treated him accordingly. Dr. Flagg's counsel attempted to show by the plaintiff himself that he had gonorrhœa, and to so build a foundation for a truthful history of the case, which, of itself alone, would be a sufficient defense. Judge Dykeman ruled that what the man previously had was impertinent, remarking, "Supposing he had had consumption?"

Dr. Upham, Jr., swore that, succeeding his father, he found the patient with acute synovitis, and treated him for it, and that he did not know of the existence of gonorrhœa until after he had been in attendance about two weeks. He thought it *only possible* for synovitis to follow gonorrhœa. His whole testimony and opinions were based on the theory of acute, idiopathic synovitis.

The experts were then called, and, one after another, by the support of Judge Dykeman, and in spite of the strong protest of Dr. Flagg's counsel, gave their testimony and opinions based upon the same false premises of acute, idiopathic synovitis. They swore that it was not to be looked for after gonorrhœa, notwithstanding such authorities as Van Buren, and Keyes, and Bumstead. Dr. Wm. T. Bull, a graduate of eighteen months, did not regard authorities at all, but said he was a law unto himself. (The marvelous wisdom of this young man is simply astounding, and yet, to look at him, no one would suspect that he was any thing else than a young man with an immense amount of conceit, and of only ordinary ability. But then appearances do so deceive.) They all swore that it is a slow, insidious disease, and that blisters, etc., were indispensable, etc.

Now, this fact of its being slow and insidious in its approach may be true enough of idiopathic synovitis, but all authorities agree that the effusion in gonorrhœal rheumatism, which this man certainly had, may take place very suddenly and unexpectedly, *e. g.*, in a night. The Judge insisted that this procedure on these false premises was all right, and overruled almost every objection of counsel for defense. He forced everything aside that at all conflicted with his preconceived false notion, viz.: that what the man had previously had, and, as shown by Dr. Upham, Jr., did have three weeks after Dr. Flagg's departure, had nothing to do with the case. Said he, "The question is, did the man have synovitis?"

The plaintiff's case closed between four and five, P M. Dr. Flagg was then called, and on the main issues contradicted every statement made by plaintiff. He testified that he *did* examine the patient at every visit except the last, and omitted it then because he was so much better that it seemed unnecessary; that he *did not* have synovitis while he treated him; that he *did not* tell him to take exercise, but simply that he might sit up and rest himself when he felt tired of bed (remarking jokingly that there was no danger of his moving about too much); that he *did* have gonorrhœal rheumatism, which was general; that he improved under treatment very rapidly, so that, when he left him, he had no fever, and was comfortable, sitting up, enjoying himself, and perfectly satisfied.

On cross-examination plaintiff's counsel asked Dr. Flagg if the three medical experts were wrong in their testimony that synovitis was a slow, insidious disease, and demanded an answer, yes or no. Dr. Flagg appealed to the Judge for the privilege to explain that, while they might be right in acute idiopathic synovitis, yet, in this particular case, it being secondary and sub-acute, they were all wrong. The Judge said he must answer yes or no. He answered they were wrong. The same thing happened to me in my cross-examination.

I was the next witness called, and testified as follows: That I found the patient, on the third day after Dr. Flagg's last visit, sitting in his easy chair, and comfortable (he had been so comfortable that he had not sent for me); that I found effusion in the knee joint which was not extensive, there being just a little puffiness on either side of the patella; that there was no pain (or but little on motion), no heat, no redness, and no swelling (save the little puffiness); that it was sub-acute synovitis, or hydrarthrosis, which was the undoubted result of gonorrhœal rheumatism; that the effusion probably took place suddenly, citing as authorities, Billroth, Van Buren, and Keyes, and Bumstead; that he did very well under treatment, and that when I last saw him, it was not acute synovitis, but simply dropsy of the joint; that my treatment had been with a view to hasten absorption by the use of iodine, gentle pressure, and the internal use of *Bryonia;* that when I called to see him after my dismissal, his knee had been blistered all over the anterior and lateral surfaces; that in my opinion the repeated blistering in that position would certainly change it from a sub-acute to an acute condition, in which Dr. Upham, Jr., says he found it; and that, if blistered at all, it should have been either a little above or below, and not directly over the synovial membrane. To this last fact Dr. Flagg had also testified.

Dr. Flagg's counsel asked to read from Van Buren and Keyes to corroborate my testimony, which, on many important points, was in direct contradiction to the medical testimony on the other side, but Judge Dykeman said, "*No.*"

When I had finished, it was about an hour past the time for the court to close. It was asked by defense that an opportunity be given in the morning to present expert and other evidence, especially to show Dr. Flagg's standing as a man of veracity, as well as his skill as a physician. Again Judge Dykeman said: "*No.*" The evidence must be closed to-night, if I have to sit till midnight;" that no evidence would be admitted in the morning, under any circumstances, and that evidence showing his skill and reputation was impertinent, and would not be admitted at all. The case was accordingly closed.

In the morning, after the pleas by counsel, came the charge to the jury. The judge said it was for the jury to decide whether or not the man had synovitis. If they decided that he did have synovitis, then it was for them to decide whether or not Dr. Flagg neglected him. If they found that he did neglect him, then the damages would be so much the greater, because of his high reputation, arguing that, if a man has a high reputation, people trust to him more confidently, and it is so much the more incumbent upon him to do to each full justice. He said nothing about Dr. Flagg's positive testimony, that while he had him under observation, he did not have synovitis. He perverted my testimony completely by saying that I swore that, when I found him, he did have synovitis, but said not a word about my explanation that neither when I took it, nor when I left it, was it acute synovitis, but sub-acute synovitis, or dropsy of the joint. He said nothing of the real essence of the matter of its being a secondary condition, which both Dr. Flagg and I had insisted upon. He made no reference to the fact that I swore that it was not acute when I left it, and that Dr. Upham, Jr., swore that, when he took it from his father (who blistered it), it was acute, and that this important link in the chain of evidence (Dr. Upham, Sr.,) was entirely left out. The jury retired, and after almost three hours' deliberation, found a verdict for plaintiff for $100.

I have gone minutely into the history of this whole affair, believing it to be of interest to each and all, as all are liable to the same attack. I ask, too, is not every reader honestly indignant at the outraged justice?

Now, Messrs. Editors, permit me just a few observations. There was no satisfactory evidence that he did go about on the knee, even admitting that Dr. Flagg had told him he might do so, proving, after all, the truthfulness of Dr. Flagg's remarks, viz: that a patient with rheumatism will not exercise too much, because exercise will hurt, and if the man isn't a fool, he will be quiet.

Let us look for a moment at the circumstantial evidence, entirely outside of Dr. Flagg's and my own testimony. It is certain that Dr. Upham, Sr., blistered the knee. Admit that this was the one thing to be done. What, then, is the inevitable conclusion? Why, certainly, that it was a sub-acute condition. Would Dr. Upham, supposed to be an intelligent physician, put a blister directly over an acutely inflamed membrane, and repeat it? Certainly not. Now what did the junior Upham find? He says he found acute synovitis. He applied ice. Admit that he was right both in his diagnosis and treatment. What is the conclusion? Inevitably, that the condition had changed, or else either he or his father was wrong; because, the condition being the same, if blisters were right, then ice was wrong; and if ice was right, then blisters were wrong; both could not be right at the same time. Why did not the plaintiff bring Dr. Upham, Sr., to testify?

Dr. Upham says that Mr. Eldridge told him of a friend of his in New York who sued a prominent surgeon for malpractice, and the surgeon had paid the amount sued for without its coming to trial. Mr. Eldridge, it will be remembered, kindly offered to allow Dr. Flagg to pay up, and save costs. He wanted to make it easy for him.

These medical experts, who are they? Dr. Stephen DeWolf, Dr. Stuyvesant F. Morris, and Dr. William T. Bull, of Chambers Street Hospital, a doctor of eighteen months' experience. Did they have a direct interest in this dirty scheme? Certainly no man of any character would leave his practice (if he had any), and spend two full days hanging around a court house to assist in blackmailing a brother practitioner, and then give unheard of opinions unsupported by the recognized authorities. No doubt they were tickled with the thought of the honor of being called as experts, and were inspired by the hope of a fat fee if the case were successful. With these considerations in view they could swear more expertly. Soon after the trial, the judge was asked to allow $250 to pay these experts for their wonderful opinions. Not half enough for such opinions from such celebrities. The court allowed them $3 apiece.

An important point: Judge Dykeman's ruling will be remembered, which excluded evidence proving Dr. Flagg's skill and reputation. When plaintiff's counsel was pleading, he read a decision of the General Term of the Supreme Court (per Mullin, Justice) sustaining the above ruling. Counsel for defense interrupted, and called attention to the continued report of the same case, when the Court of Appeals had reversed that *identical case* on that *identical point*, viz.: because such evidence was not admitted. Judge Dykeman examined the reports. In his charge he said nothing about this fact, until asked by counsel for defense to charge the jury that such evidence was important for their consideration, and, in spite of his former opposite rulings, excluding all such evidence, he said: "They are so charged."

A strange coincidence: When the clerk called the roll of impaneled jurors for this case, it was noticed that to one name there was no response; yet there were twelve men in the jury box. The clerk could not understand it, and again called the roll, but with the same result. He was about to investigate the matter, when the missing man appeared from the back part of the court room, and at the same time a man known to be a client and neighbor of Mr. Keogh (plaintiff's counsel), quietly left the jury box. Why was that man there? And why did he persist in staying there until it got so uncomfortably hot? Moreover, it was predicted by a friend of Dr. Flagg's, who knew some of the jurors and their relations with Mr. Keogh, that they would never bring in a verdict against Mr. Keogh.

I could mention other incidents, equally contemptible and outrageous, but I have already spun my yarn too long.

Dr. Flagg has the hearty sympathy of a host of friends; in fact, of every decent person who knows the circumstances. Some are so anxious that he should appeal it that they have offered to help pay the costs; but he considers it a privilege as well as a duty, both to himself and to the profession, to carry it up, and to place this blackmailing viper and his accomplices in their proper light before the public, and so to lessen the danger of like assaults upon the profession by similar speculators, who might be encouraged by this fellow's success (as he was by his friends), providing Dr. Flagg should submit to this decision.

Therefore, the case will be carried to a higher court, and in due time the result will be made known in the columns of your journal. I am yours sincerely,

R. Oliver Phillips, M. D.

Yonkers, N. Y., Oct. 30, 1879.

Dr. Burr G. Carleton has removed to 166 West 34th St.; Dr. Geo. Vandenhoff to 232 West 45d St.; Dr. Geo. E. Belcher to 522 Madison Ave.

BIBLIOGRAPHICAL.

THE ENCYCLOPEDIA OF PURE MATERIA MEDICA: A Record of the Positive Effects of Drugs upon the Healthy Human Organism. Edited by Timothy F. Allen, A.M., M.D. Boericke & Tafel, publishers; New York.

Our first glance at these ten stately volumes sufficed to convince us that they might truly be styled a "monument"—at all events, to the enterprise and liberality of their publishers. We heartily wish the liberality and enterprise of the publishers had been engaged in a better cause than in presenting in clear and beautiful typography and substantial binding the mass of trash, of wild vagaries, of symptoms which seem to have been gathered at random from every language under heaven, from every insane asylum in the land, and from nurseries where fond mothers take seriously to heart the symptoms and sayings of their young offspring. Mixed with all this trash, the trained searcher may possibly find the real gems of our therapeutics, for they are there; but they are often so covered with what is perfectly worthless, that a special training is necessary to evolve them from surrounding rubbish. We are inclined to think this work will stand for something more than a "monument" to the liberality of the publi-hers; it will also form a "monument" to the folly of a school, claiming to be scientific, which could receive with strong expressions of commendation—as this book has been received by many of our medical journals—a book like this. There was a time when our school, with its scientific basis and its earnest common sense workers, might have conquered the medical world, but, by just such folly as we find in this work, such grave consideration of ridiculous symptoms and transcendental nonsense, it has done more to fritter away its power, and clip its own wings, than any opposition by our opponents. It gives us no pleasure to speak in this manner, and we turn from the expression of individual opinion as to the general scope of the work, to a careful scrutiny of the editor's translations of Hahnemann. We have a right to expect here scholarly work, and if we find a simple translation, from so important a writer as Hahnemann, full of errors, what reliance can be placed upon any of the editor's work? A sentiment of reverence for the great founder of our school, and a due sense of literary responsibility, ought surely to have incited Dr. Allen to put forth his best efforts in this particular department of his editorial labors. Nor can we conceive what valid excuse he could offer in the event of failure. Unlike his predecessor in the same field, he has not been compelled to hurry over it under the stimulus of poverty and meagre remuneration. He has occupied for years the chair of materia medica in a leading homœopathic institution. He has received a classical education. He enjoys the reputation of an accomplished German scholar; and is able to command, in aid of his literary enterprises, the counsel and cooperation of some of the leading veterans of his school. Moreover, he has publicly announced that while, of necessity, much of the work on the Encyclopedia has been confided to the hands of subordinates, *all the translations from the German have been executed, and finally revised, exclusively by himself.* Surely, then, it was not too much to expect that, in this highly vaunted publication, we should be presented with what we have never had before—*the exact meaning of each one of Hahnemann's symptoms, in clear and correct English.* If this shall be found to be the case, there will be ground for assuming that the authorities throughout have been equally well treated. Reasoning thus, we took up the seventh volume, and opening at the article "*Nux vomica*," proceeded to gather together the extracts from Hahnemann's "*Materia Medica Pura*," arranged therein under the rubric "Mind;" seventy-six symptoms in all. Each of these we compared carefully with the original text. The ensuing columns display at a glance the more striking results of our investigations:

Hahnemann.	*Literal Translation.*	*Allen's Translation.*
S. 1238. Sie hält den *gegenwärtigen* Schmerz für unausstehlich, etc.	She regards the *present* pain as intolerable, etc.	S 6. The *usual* pain seems intolerable, etc.
S. 1236 Zanken, Vorwürfe, Schimpfreden, eifersüchtige Schmähungen, mit unzüchtigen Ausdeücken gemischt, etc.	Quarrels, reproaches, scolding, insults from jealousy, mingled with lascivious expressions, etc.	S. 8. *He* quarrels, reproaches, scolds, *mingled with* unchaste expressions, etc.
S. 1261. Sie kann die mindeste Widerrede und auch die *vernünftigsten Vorstellungen* nicht ertragen, etc.	She cannot bear the slightest contradiction, or even the most *reasonable remonstrances*, etc.	S. 17. She cannot tolerate the slightest contradiction, or even the most *gentle persuasions*, etc.
S. 1253. Sie kann sich selbst über die kleinsten *Uebel* nicht hinwegsetzen.	She cannot disregard the smallest *evil* [misfortune.]	S. 20. She cannot endure the slightest *ailment.*
S. 1257. Sie stöhnt und ächzet jämmerlich, ohne eine Ursache *anzugeben.*	She moans and groans piteously, without *stating* a cause.	S. 23. She moans and groans piteously, without any *assignable* cause.
S. 1254. *Aengstliche* Bedenklichkeit und Untröstlichkeit, etc.	*Anxious* solicitude and unconsolableness, etc.	S. 25. *Extremely* solicitous and inconsolable, etc.
S. 1237. Nach Mitternacht sehr heftiges Herzklopfen mit äusserster *Angst, welche ihn* zur Selbst-Entleibung *treibt.*	After midnight, very violent palpitation, with extreme *anxiety, which impels* him to suicide.	S. 37. Extreme anxiety, with violent *palpitation, which impels* him to suicide.
S. 1234. Früh *beim Erwachen*, und Nachmittags, Angst, etc.	In the morning, *on waking*, and in the afternoon, anxiety, etc.	S. 41. Anxiety, etc., in the *morning and afternoon.*
S. 1239. Angst, mit *Trieb*, sich selbst zu entleiben.	Anxiety, with *impulse* to commit suicide.	S. 42. Anxiety, with *desire* to commit suicide.
S. 1256. Angst aus verdachtsamer und befürchtender *Bedenklichkeit*, besonders in den *Nachmitternachtstunden.*	Anxiety caused by a suspicious and apprehensive *doubtfulness* [scrupulosity], especially after midnight.	S. 45. Anxiety, caused by a suspicious and apprehensive *condition of mind*, especially during the *afternoon* hours
S. 1262. *Er* ist ärgerlich *bedenklich*, nimmt alles übel, etc.	*He* is fretfully *scrupulous*, takes everything ill, etc.	S. 57. *She* is peevish, *thoughtful*, takes everything ill-naturedly. etc.
S. 1264. *Zornige* Aergerlichkeit, Zornmüthigkeit.	*Angry* fretfulness; irascibility.	S. 58. *Scornful*, peevish, inclined to be angry.

Hahnemann.	*Literal Translation.*	*Allen's Translation.*
S. 1295. Des Morgens, Scheue vor solcher litarischen Beschäftigung, bei welcher man selbst denken und selbst die Ideen aus sich *entwickeln* muss, um sie entweder schriftlich aufzuzeichnen, oder mündlich vorzutragen, etc.	In the morning, dread of that kind of literary work at which one has to think, and to *develop* one's own ideas, in order either to put them into writing or to deliver them orally, etc.	S. 73. Dread of that kind of literary work, at which one must think, and *employ* the ideas, whether to be *elaborated* by writing, or to be delivered orally, etc.
S. 1293. Es hindert ihn, er weiss selbst nicht, was, vorzüglich den *wissenschaftlichen Beschäftigung.*	He is especially hindered, he knows not why, in his *scientific work.*	S. 78. He is especially hindered in his *scientific experiments;* he himself does not know why.
S. 1294. *Unaufgelegtheit* zu Kopfarbeiten, etc.	*Disinclination* to mental work, etc.	S. 74. *Inability* for mental work, etc.
S. 1284. *Zu allen Unternehmungen und Geschäften* träge; sie ermüdet gleich.	Indolent *at all undertakings and occupations;* she soon wearies.	S. 79. Indolent at every *undertaking in business;* she soon becomes weary.
S. 1297. *Unfähig, gehörig zu denken,* verspricht er sich oft im Reden, etc.	*Unable to think properly,* he often makes mistakes in speaking, etc.	S. 84. Frequently makes mistakes while talking, etc.

Here, on enumeration, we are confronted, in the 3d column, with 19 instances of downright mistranslation, errors of omission, or bad English which impairs the sense—*i. e., one quarter* of Hahnemann's symptoms, from a single section of this most important remedy, have been inadequately rendered by the encyclopedic editor! On comparing the late Dr. Hempel's translation of the same symptoms, we find that, though by no means faultless, and in part only a loose paraphrase of the original, it is less frequently inaccurate than Allen's, while, in point of style, it displays a marked superiority; for Hempel's English, though he was by birth a foreigner, is always lucid and grammatical. What sort of English Dr. Allen writes is partially shown by the above citations, but will be made more apparent if we examine his mode of rendering some of Hahnemann's longer and more involved constructions. Take, for example, the following from *Opium,* vol. VIII. of the Encyclopedia:

Hahnemann.	*Literal Translation.*	*Allen's Translation.*
S. 27 (Note.) Die Geistes- und Gemüthssymptome lassen sich beim Mohnsaft nicht so genau von einander trennen, wie bei andern Arzneien, *und* die erstern zu Anfange bei den Kopf-symptomen, die andern zu Ende aller andern Zufälle stellen, weil sie bei Mohnsafte beide gewöhnlich sich zusammenpaaren. Wenn Mohnsaft zur *palliativen Unterdrückung* der Schmerzen, *der Krämpfe, des entgegengesetzten Geistes- und Gemüthszustandes* (wie in S., etc.), oder auch zur Vertreibung des naturgemässen Nachtschlafes (in letzterm Falle *gewissermassen* homöopathisch) gebraucht wird, so bringt er an der Stelle gewöhnlich *solche* Extasen des Geistes und Entzückungen des Gemüths hervor—alles schnellvorübergehende Entwirkung Diese Extasen und Entzückungen kommen dem innern verklärter Erwachen der Somnambulen (Clairvoyance) oft sehr nahe.	The mental and emotional symptoms of Opium cannot be so accurately separated from each other as those of other drugs, *and* the former placed at the beginning, with the head-symptoms, the latter after all the other symptoms; because, in Opium, the two are commonly associated. When Opium is used for the *palliative suppression* of pain, *of spasm, of the opposite* mental and emotional condition (as in S., etc.), or for the prevention of the natural night-sleep (in the latter case, *in some sort,* homœopathically), it usually causes instead *such* mental ecstasies and emotional transports; all this being a very transient primary effect. These ecstasies and transports are often very like the internal bright awaking of the somnambulists (clairvoyance).	S. 152 (Note). The mental and emotional symptoms of Opium can not be so well separated as those of other drugs, *when* the former are placed at the beginning, with the head-symptoms, the latter after all the other symptoms, because, in Opium, the two are commonly associated. When Opium is used for the *palliation* of pain, *of spasm in* the opposite condition of mind and disposition (as in S., etc.), or for the prevention of natural sleep (in the latter case, *most certainly* homœopathically), it usually causes in their stead, *a* mental ecstasy and emotional excitement—a very transient primary effect. This ecstasy and excitement are frequently like the internal clearness of somnambulism (clairvoyance).

Here we see that whereas Hahnemann divides his note into two paragraphs, corresponding with two obviously distinct statements respecting the action of the drug, Dr. Allen runs them both together, as if they were continuous. Next, by a careless use of the conjunction "when," he completely alters the meaning of the first sentence, or, rather deprives it of any meaning whatever. In the second sentence, not content with omitting the important word "suppression," he continues to make Hahnemann talk nonsense; and, lastly, without the slightest warrant, converts a qualified parenthetical statement into one most emphatically positive. And all this within the compass of nine printed lines!

Surely, the sage of Coethan would have been far more worthily treated, if Dr. Allen, instead of "cribbing" here and there from Hempel's translation (as a comparison of their blunders shows that he has done), had boldly adopted it throughout. But, lest we should seem unjust to the "responsible editor," in confining our attention to a limited section of his work, we copy, in addition, the following, from "*Pulsatilla,*" vol. VIII.:

Hahnemann.	*Literal Translation.*	*Allen's Translation.*
S. 1,108. * * * am Tage ebenfalls Angst, *mit Zittern und Gefühl* von *fliegender* Hitze am ganzen Körper, etc.	* * * during the day, anxiety, *with trembling, and sensation of flying* heat over the whole body, etc.	* * * during the day, anxiety, *and flushing* heat over the whole body, etc.
S. 1,141. Es ist ihr so still in Kopfe und alles so leer umher, als	Her head feels as quiet, and everything about her feels as empty, as	It seems so quiet in her head, and everything feels so empty, that she

Hahnemann.	*Literal Translation.*	*Al'en's Translation.*
wenn sie allein in Hause und in der Welt wäre; sie mochte mit Niemandem sprechen, *gleich als wenn die Umgebungen ihr nichts angingen und sie zu Niemand gehörte.*	if she were alone in the house and in the world; she wants to speak to no one, *just as if the surrounding company were nothing to her and she belonged to no one.*	seems alone in the house and in the world; she wants to speak to no one, *just as if her surroundings did not exist, and she paid attention to no one.*
S. 576 (Note.) Bei mehren *ältlichen Personen, etc.*	In several *elderly* persons, etc.	In several *older* people, etc.
S. 645. (Note.) * * * so wie die längere und verstarktere Bewegung bei Pulsatille nicht weniger, als das lange Sitzen *Symptome erregt*, welch jedoch. wenn man wieder zu Ruhe und zum Sitzen kommt, erst recht fühlbar zu werden pflegen.	* * * so also longer continued and stronger motion *excites symptoms* in Pulsatilla, no less than long sitting, which [symptoms], however, are only accustomed to become really perceptible if one again becomes quiet and sits.	* * * so also longer continued and more violent motion *causes aggravation* no less than long sitting still, which, however, *are* only really felt and noticed after one has again sat down and become quiet.

Last of all, we invite attention to what is undeniably the gem of the present collection, also to be found under *Pulsatilla.*

Hahnemann.	*Literal Translation.*	*Allen's Translation.*
S. 743. (Note) Die Symptome der Pulsatille wechseln auch in Rücksicht der Tageszeit, wo sie zu entstehen und in der sie anzuhalten pflegen. *Die Haupttageszeit für dieselbe ist der Abend, hienächst* die Stunden bis zu Mitternacht. *Seltener* ist die Entstehungszeit der Pulsatille symptome Nachmittags um 4 Uhr, nach seltener früh, u. s w.	The symptoms of Pulsatilla alternate in respect to the time of day at which they are accustomed to arise, and at which they cease. *The principal time of day for them is the evening; next to this,* the hours until midnight. Pulsatilla symptoms *more* rarely arise about 4 P. M: still more rarely in the morning, etc.	The symptoms of Pulsatilla alternate in respect to the time of day when they appear and disappear. THE PRINCIPAL TIME FOR THEM IS IN THE EVENING AND SUCCEEDING HOURS TILL MIDNIGHT. *Symptoms are seldom noticed about 4 P M., and still more seldom in the morning.*

This establishes the almost incredible fact that a professor of materia medica in the Homœpathic College of this metropolis is so far from knowing exactly what is the leading aggravation of Pulsatilla in respect to time, that he gives a grossly incorrect translation of the very sentence (marked by himself as most important), in which Hahnemann has defined that aggravation in the plainest possible language! Note, also, with what distinctness of graduation the four periods are indicated in the original, and how the editor of the Encyclopedia has treated this descending scale.

Having thus obtained a sufficient number of "crucial" indications, to what conclusion do they lead? To none whatever, that we can discern, which does not imply the utter condemnation of Dr. Allen's work—*in so far as Dr. Allen himself is actually responsible for it.* The notes by Dr. Hughes throw interesting light upon Hahnemann's employment of authorities, and the precise import of their utterances. Other prominent contemporaries have aided efficiently in imparting value to the few original contents of the volumes. But with the great mass of these contributions the so-called "responsible editor," has really had nothing to do but insert them in their proper places. And as to what he has adopted from French sources, we happen to know that he is incapable of even properly revising it. The one sole portion of this imposing structure which he can justly claim as the product of his personal and unaided abilities, is that portion which we have inferentially shown to be honeycombed with error, and defaced with all the signs of heedlessness and incompetence.

We are far from demanding that the amount of evidence here adduced shall be deemed all-sufficient in the case. If thought proper, let the final verdict be preceded by a more extended examination of the *corpus delicti*, especially the dishonored fragments of the Hahnemannian remains.

We shall await the result in full confidence that it will justify the sentence we have foreshadowed, and relieve the Homœopathic profession of any further responsibility in connection with Dr. Allen's gigantic and most discreditable *fiasco.*

SANITARY ENGINEER. Issued on the 1st and 15th of each month. Subscription price, $2 per year; $1 for six months; single copies, 10 cents. For sale by all enterprising newsdealers. Published at 140 William Street, New York. P. O. Box, 3,037.

An eminent English physician remarks that if every medical man would *examine the drainage of his own house*, and learn to what extent most people dwell over pent-up pestilence, *we should have an army of sanitarians, earnest and keen to ferret out unsuspected sources of illness.*

Every physician should therefore read the *Sanitary Engineer*, which was established, in 1877, to enlighten the public regarding questions of house drainage, ventilation, heating, lighting, water supply, and public health. It contains matter for the householder, as well as for the professional and practical man, and is interesting to all people who either build houses or who live in them.

Its subscribers include architects, chemists, civil and gas engineers, health officers, physicians, officers of public institutions, with managers of large public and private corporations, and intelligent householders generally.

It employs the ablest specialists as contributors, and it tries to discuss all topics with accuracy and fairness.

Among its regular contributors, Col. Geo. E. Waring, Jr., of Newport, and Edw. S. Philbrick, C. E., of Boston, write upon sanitary engineering subjects; Prof. Henry Morton, president of Stevens Institute, treats topics connected with chemistry, and during the past year has very fully discussed the electric light, water gas, and food adulteration; Robert Briggs, C. E., of Philadelphia, and Dr. John S. Billings, U. S. A., vice-president of the National Board of Health, have in charge subjects connected with ventilation and heating; "School Hygiene" is the specialty of Dr. D. F. Lincoln, of Boston, and Hon. John D. Philbrick, LL. D., U. S. commissioner at Vienna and Paris Exhibitions. Tenement house reform has been written on by Chas. L. Brace and others. In a like manner other topics have been treated by competent men.

It already has paying subscribers in thirty-four States and Territories of the Union, as well as in Canada, Mexico, England, Ireland, Scotland, France, Belgium, Germany, Austria, Russia, Switzerland, Denmark, India, Japan, Australia, New Zealand, and the Sandwich Islands.

Full details of the terms of a $500 competition for a model school will be found in the November 1st issue, together with the first of a series of articles by Dr. D. F. Lincoln, of Boston, and other experts, on sanitary school construction.

Questions are solicited from persons seeking information on any of the subjects treated by this journal, which will be answered through its columns without charge, when they are of general interest. Inquiries of this kind from physicians will receive prompt attention.

OBITUARY.

Dr. Erastus A. Munger, one of the most widely-known and esteemed of the physicians of Oneida county, died Nov. 4th, at Waterville, N. Y. Dr. Munger was born in Copenhagen, Lewis county, February 12, 1813. He commenced the study of medicine in the office of Dr. S. G. Haven, in Waterville, with whom he remained two years. In 1834, at the age of twenty-one, he was licensed by the "Allopathic Medical Society of Oneida County," and commenced practice in Sauquoit. In the fall of the same year he entered Jefferson Medical College in Philadelphia, and graduated in March, 1835. He immediately commenced practice in Waterville, where his parents resided, and where he had spent most of his boyhood. In the summer of 1843 he went to New York for the purpose of learning something about what was then a new system of practice, homœopathy, and through the influence of Drs. Gray, Freeman, Bayard and Kirby, he espoused the new practice, and returning home, became the first homœopathic physician in Oneida County, there being none other in the vicinity nearer than Syracuse. The public at that date knew little of homœopathy, and probably no physician withstood more opposition, slander, ridicule and abuse than he when in the early days he stood alone as believer in, and patron of homœopathy. He became a member of the Homœopathic Medical Society at its organization, and was its first president. In 1848 he was elected a member of the American Institute of Homœopathy, in 1862, vice-president of the Homœopathic Medical Society of the State of New York, and in 1864 president of the State Society. In 1872 he was appointed by the regents of the university as a member of the State Board of medical examiners. He resigned the office on account of inability to attend the meetings of the Board.

A special meeting of the Oneida County Homœopathic Medical Society was held to take action on the death of Dr. E. A. Munger. Drs. Wells, Watson, and Gardner were appointed a committee to draft appropriate resolutions, which were adopted as follows:

Whereas, The members of this society have heard with deep regret the announcement of the death of Dr. E. A. Munger, of Waterville, Oneida County, therefore

Resolved, That by the death of Dr. Munger, this society has lost one of its most distinguished members, who has filled with credit to himself and honor to the profession the office of president of the Homœopathic Medical Society of the State of New York, and of this society.

Resolved, That the medical profession of this county has lost one of its brightest ornaments, and the cause of homœopathy one of its most zealous advocates and defenders.

Resolved, That we desire to record our appreciation of his rare professional ability, as exhibited in a most successful medical career, and his earnest usefulness in advancing the interests of our profession.

Resolved, That we tender to the family of the deceased our most heartfelt sympathies, and that a copy of these resolutions be entered upon the minutes of this society.

Resolved, That the members of this society will attend the funeral.

Eminent as a physician, respected as a citizen, a man of sturdy and outspoken convictions, his death leaves a peculiar void in the community. His health had been gradually failing for the past few years, and the strain incident to so large a country practice enfeebled his constitution, till, yielding reluctantly his practice to his partner, Dr. George Allen, he died from general debility rather than from any particular disease.

SCOTT-WRIGHT.—Emma Scott-Wright, M.D., died in Brooklyn, Nov. 17, of tubercular phthisis, after an illness of eight months. She was born in St. Louis, Mo., in 1848; studied medicine in the N. Y. Med. College and Hospital for Women, where she graduated in 1871; and was subsequently appointed lecturer on theory and practice in the same institution.

She was the founder of the N. Y., Homœopathic Dispensary for Women and Children, in East 23d St., which, mainly through her efforts, is left in a flourishing and self-supporting condition.

REPORTS OF SOCIETIES, ETC.

GYNÆCOLOGICAL RETROSPECT.

BY GERTRUDE A. GOEWEY, M. D.

PART I.

Read before the Hom. Med. Society of Kings County, N. Y.

In preparing a review of gynæcological literature, I have endeavored to call your attention to that which is most interesting, and present it in as concise and condensed a manner as possible from the various subjects selected.

Dr. Garrigues offers some very interesting remarks on gastro-elytrotomy, in which he points out the superior advantages of the operation over Cesarean section.

As early as 1806 the German obstetrician, Joery, proposed Cesarean section, but his plans were never executed. In 1820 Ritgen advised a plan, and in 1821 he operated upon a woman of narrow pelvis, owing to osteomalacia, but on account of the hemorrhage that followed the incision, he performed Cesarean section, saving the child. Ritgen: Die Angeigen der mechanischen Hoelfen bei Entbindemgen. (Giessen, 1820, p. 406.)

Baudelocque performed the operation in 1844. Nouveau procede pour pratiquer l'operation Cesarienne. (Thése de Paris.) This is the only case of gastro-elytrotomy on record, before Thomas. "Gastro-elytrotomy, a substitute for Cesarean section." (Am. Jour. Obst., vol. III., No. 1, May, 1870.)

With reference to the safety of the operation, Dr. Thomas makes this statement; that in New York, within the past two hundred and fifty years, there has been performed only one successful operation of Cesarean section, in which both mother and child were saved; while gastro-elytrotomy has been performed six times within eight years with three successes. Surely such evidence as this must be strongly in favor of gastro-elytrotomy.

Most works on obstetrics do not even mention the operation, and it was lost sight of until Thomas in the second period of its history (1870) invented it in an improved form, not knowing at that time that the operation had ever been tried.

His procedure of operation is precisely the same as Ritgen's, with this exception, that he tears the vagina,

after he has cut enough to admit the tips of his fore-fingers.

The theory he advances is, that the arteries of the vagina do not bleed as much when torn as when incisions are made. He sees no reason of interfering with large iliac vessels as Baudolocque did.

His aim is to make a longitudinal rent, therefore he tears toward the os uteri, and toward the descending ramus of the pubes. One of Thomas' cases was a dead woman from whom he delivered a living child. Skene: "History of a case of Gastro-elytrotomy." (New York Med. Jour., 1874, vol. XX, p. 401, seq.) In one of his cases the child was dead before the operation, but the mother was saved. In the last one, both mother and child were saved. "Gastro-elytrotomy successfully performed." (Am. Jour. Obst., vol. VIII, February, 1870.) "A second successful case of Gastro-elytrotomy." (ibid., 1878, vol. X, p. 628, seq.)

Intimately connected with this question is the possibility of operating on the left-side. Dr. Skene does not think it feasible, while Thomas expects to operate on his second patient on the left side. (Am. Jour. Obst., April, 1878, vol. XI, p. 245.) Baudolocque operated on the left side. The practical lesson conveyed from the history of his cases is, to avoid all sharp instruments.

Ritgen tied only the epigastric artery, where Thomas and Skene, whose incisions extended toward the mesial line, only forcibly compressed the superficial epigrastric. The actual cautery of some kind renders the incision of the vagina comparatively safe. If the rent extends into the bladder, experience has shown—Dr. Thomas' second case and Dr. Skene's third case—that the fistula may heal of itself.

Injections of lukewarm milk into the bladder immediately after operation has been advised to ascertain if fistula exist; if found, the fissure can be closed by a few stitches.

The most recent statistics of Cesarean section collected by Mayer show, in 1,665 women, a mortality of fifty four per cent. the same figure was obtained by Michaëlis, while Kayser even shows sixty-four per cent. (Spiegelberg, Geburtshülfe, Yahr., 1878, p. 852.)

Dr. Robert P. Harris, of Philadelphia, has gathered eighty-nine operations performed in the United States; fifty four per cent. of the mothers died, and of the children, fifty-one per cent.

In the Lying-in-Hospital of Vienna during this century not a single mother survived. Dr. Thomas has found that since the incorporation of Nieuw Amsterdam in 1621, only once have both mother and child survived in New York. "Comparison of the results of Cesarean section and Laparo-Elytrotomy in New York" (New York, Med. Jour., May, 1878. Statistics ought to induce obstetricians to give gastro-elytrotomy a fair trial. The dangers are pointed out to be less in gastro-elytrotomy than Cesarean section. The sequel of gastro-elytrotomy is cellulitis, but is not so fatal as peritonitis. Recommended in certain cases instead of craniotomy or embryotomy. Parey asserts that craniotomy gives no better results than Cesarean section where the conjugate diameter is two inches and one-half or less. "The Comparative merits of Craniotomy and the Cesarean Section in Pelves with a Conjugate Diameter of Two and a Half Inches or less." (Am. Jour. Obst, vol. V, p. 644.) The author of the paper says, gastro-elytrotomy is not to supplant any acknowledged obstetric operation. The question is only, if it ought not to be admitted among regular operations, and in what kind of cases ought it to be tried.

Thomas substitutes the term laparo-elytrotomy for gastro, in his last publication on the subject.

The surgical treatment of stenosis of the cervix-uteri by Dr. Sims, is a very interesting and lengthy article, containing forty-eight pages.

Stenosis of the cervix uteri has long been recognized as a cause of dysmenorrhœa and sterility.

Professor McIntosh, of Edinburgh, was the first to demonstrate a method of treatment and to point out its influence upon menstruation. He employed graduated bougies for dilating the cervix, but his followers never succeeded in achieving the victory in adopting his method. Simpson found that the cure was not permanent, and employed bilateral incisions of the cervix, and never uses tampon to arrest hemorrhage. He prefers per-chloride of iron and glycerine, by painting with camel's hair pencils, several times into the vagina.

Sims found that alarming hemorrhage followed Simpson's method, and therefore made antero-posterior incisions, inst ad of the bilateral, and objects to the use of iron as a styptic, but returns to the use of alum suggested by Dr. Thomas T. Pratt, of Paris.

It has been found to be just as effectual in forming a consolidated immovable mass to hold the plug in the cervix, and the odor is not so disagreeable as when iron is used. He recommends saturated solution of alum (one to twelve) with carbonic acid added (one to forty), and after the plug is in the cervix, he tamponed the vagina with cotton wool alumnized. Sims claims that his operation is only applicable to cases of flexion where the intra vaginal portion is unequally developed, and the posterior segment is longer than the anterior, and in these cases there is more or less ante flexions. His present method of operating is to conjoin incisions (bilateral) with dilatation, using his metrotome and trivalve uterine dilator. He forcibly dilates, until the cervix is large enough to receive the cervical plug, which he has made of hard rubber, glass, sometimes silver or aluminium, graduated according to English measure. Since adopting the plug system he has had but two cases of hemorrhage, and those he claims were his own fault.

In one case the patient objected to being catheterized, and on the third day hemorrhage occurred; the tampon had slipped and removed the plug from its proper position. His explanation is that there was no hemorrhage when operated upon, and did not fear any; he only put tampon enough to fill the upper half of vagina, and the efforts of the patient to urinate displaced the tampon, and hence the plug slipped down. From his experience with this case, and a similar one, he comes to this conclusion, that if the plug or tampon slips down, the success of the operation may be jeopardized. The patient is kept in bed, may lie on either side, but is not allowed to rise, and catheter must be used. On the sixth day the plug and tampon may be removed; sometimes part is removed on the third or fourth day; in two cases he removed in twenty-four hours on account of fever and rigors that followed, but these symptoms pass off on removal of tampon.

The paper establishes two points: that bilateral incision belongs to Simpson, and is suitable for stenosis when the intra-vaginal cervix is normally developed, and the two segments symmetrical; and that antero-posterior incisions, when the intra-vaginal cervix is abnormally developed; when the posterior segment is two or three times as long as the anterior, and associated with ante-flexion, is Sims' method. The discussion was participated in by several eminent physicians. Dr. Fordyce Barker claimed that operation was sometimes performed unnecessarily, and was becoming more rare than formerly, and he did not believe that so many patients were cured as reported, and that the effectual cure should not be attributed to a few months' observation, but to years'. Emmet says when the flexure is above the vaginal juncture and circulation obstructed, the operation is not successful.

The first point is to determine if the flexion is due to old attacks of cellulitis, and if the cause can be determined, the operations for stenosis will pass out of sight. He claims that mechanical dysmenorrhœa depends

upon defective nutrition somewhere beyond the uterus.

Dr. Noeygerath, of New York, spoke of Dr. Peaslee's objections to the bilateral and antero-posterior operations. The object of the operation is to strengthen the cervical canal; in order to do this the cut would extend into the peritoneal cavity, which he objects to. The second objection is that the rent was made into the parenchyma of the uterus where large numbers of lymphatics and veins were found, and often resulted in phlebitis and lymphangitis.

He therefore incised the inner and outer os just enough to give it the normal calibre, and only cut through the mucous membrane. He also claims that many cases that are reported effectually cured, relapsed into the same condition as before, after eight or ten months. He could not see that one man in twenty years' practice would meet with nearly a thousand cases where the cervix-uteri required incision, and other physicians whose gynæcological observations were equally as large, found occasion to perform the operation in one-fifth the number of instances.

Dr. Wilson, of Philadelphia, was no advocate of either Simpson's or Sims' operation. He preferred dilatation, and success was permanent.

Dr. Reeve relates a case of rupture of the perineum without implication of the vulva. Mrs. S., aged thirty-three, was attended by a German midwife, in her third confinement. On the 10th day he saw the patient and found enormous laceration of the perineum.

The rent was near the junction of the upper fourth with the lower three fourths of the labium (right side), extending to the outer boundary of the labium downward, and crossed the perineum to the rectum; both the anal sphincters were divided, the laceration being an inch and a half. Nelaton's practice was resorted to by bringing the parts together with four silver wire sutures. His hopes were not realized; granulations did not take place. On the 21st day he proceeded to operate. Two wire sutures were put in the recto-vaginal septum, two through the labium, and two deep ones through the perineum, the last four being quill sutures. To each end was attached a short piece of bougie.

The strain on the stitches being unequal, it was found that the short pieces of bougie were a great improvement. This method was resorted to before Grailly Hewitt introduced his bead suture, in which the wires were attached to gutta-percha beads.

The sutures were removed on the 27th day, and union had taken place except just above the internal sphincter ani, where a small fistula was found.

On March the 4th he operated upon this recto-vaginal fistula, not by forced dilatation of the anus, as is recommended by Thomas, for fear of laceration.

His mode of operating is not stated, did not prove successful, and the patient passed from under his observation. The case is not mentioned for its successful operation, but for its great rarity of this, or any variety of central rupture of the perineum in a multipara. If failure of success depended upon not dividing the sphincter ani, he followed such authority as Hegar and Kaltenbach. (Die Operative Gynäkologie, Erlangen, 1874.)

There are only two cases reported in medical literature where a child was delivered by the rectum, the vulva not being implicated, and no particulars are given of these cases. The first is by Blundell, the second by Byford. An interesting case by Dr. Goodell, in which an arm protruded from the anus of a primapara after the delivery of the head, is worthy of note in this connection. (Gynæcological Trans., vol. I, p. 313.)

Dr. Boeur saw a case where two sphincters were torn. In one instance the child passed through the septum into the rectum and through the anus.

Considering the direction of the forces, the relations of the parts, and the protrusion of the head, it is somewhat singular to me that the child does not more frequently make its exit through the anus.

That central rupture takes place, leaving both anal vulvar openings unmolested, is another point of interest. Several cases are reported by Churchill, Velpeau, and Busch, while Moser gives thirty-three cases. Thirty-five cases of central rupture of the perineum occurring in primipara (which is usually the case) are reported by different authors. By medical research there is found but one fair case occurring in a multipara, and the author claims that his case, now reported, stands alone. Duparcque relates a case, second labor, where there was a resisting cicatrix at the fourchette. This modifies the circumstance accompanying the labor. The etiology of the subject can be found in systematic obstetrical works. The investigation as to the frequency of central perineal rupture is interesting. Birnbaum maintains the idea that there must be predisposing and determining cause to give rise to the accident.

Eminent authorities say it arises from too great laxity of the perineum. Then this question arises: Why is it confined to primipara instead of multipara?

Roux says rigidity of the perineum favors the accident, or it may be too thin and membranous. So late a writer as Olshausen expresses the same opinion, or if the perineum is too broad, the head is directed forward and bores down into the peritoneum; this structure envelops the head like a cap, and on account of extreme thinness it bursts, and central rupture occurs more frequently when this condition is found. Dr. White, of Buffalo, says, in the discussion that followed, that he makes a triangular division of the perineum to prevent rupture, and that the surfaces heal readily, and thinks this procedure should be more frequently resorted to in rigidity of the perineum.

The perineum was not more lax than usual in the case reported.

He regrets that, having failed to operate immediately, a greater length of time should have been allowed before the operation.

Extra-uterine pregnancy, with discharge of the fœtal bones through the bladder, by Dr. White of Buffalo, possesses several points of interest. The case was first diagnosed as pelvic hematocele. The aspirator was used, and three or four ounces of straw-colored fluid were drawn off; peritonitis followed; subsequently found a recto-vaginal fistula. The operation upon this fistula proved successful, and in the course of a month a foreign body was expelled, which, upon examination, proved to be bone, the shape and size of a fœtal rib about the fourth month; other bones were frequently removed per urethra, by instrumental assistance. The bones were all clean and white except the last, which was covered with black calcareous deposit, not easily removed.

A solid body still remains, which is supposed to be the skull encysted, bound down by adhesions. The bladder has resumed its normal condition. The location of this fœtal tumor in extra-uterine pregnancy is rarely met with, namely, in front of uterus, and anterior to the bladder, as the discharge of the remains would indicate.

The discharge of fœtal remains through the bladder is, according to Parry, one of the most infrequent (being but two or three per cent.) of the various methods by which they may be disposed of.

Of spontaneous elimination, those through the intestinal canal, vagina, and bladder more frequently prove fatal. Parry asserts that no surgical interference should be resorted to in extra-uterine pregnancy until the symptoms demanded. The opinion prevails that where an incision is made in the abdomen to remove fœtus, the placenta should not be removed. Another case is related where the bones all pass off through the rectum in the course of three months. Since then this woman has had two children. Dr.

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Attee reports a case where the fœtus was extracted, placenta left. What becomes of the placenta is not discussed.

Attention is called to the treatment of acute parenchymatous nephritis of pregnancy.

Out of one hundred and fifty-six patients admitted to the Boston Lying-in Hospital, albumen was found in sixty-four, or nearly forty one per cent.

The etiology is still *sub judice*; the treatment still a vexed question. Recognized authorities can throw but little light upon the subject.

Professor Playfair advises induction of labor where the amount of albumen is large and increasing. (System of Midwifery, 1878, p. 195.)

Professor Barker considers this procedure only justifiable when treatment has been thoroughly tried without success. (Am. Jour. Obst., July, 1878, p. 558.)

Schroder, Manual of Midwifery, 1873, p. 319, says all obstetrical manipulations are contra-indicated so far as maternal interests are concerned, although the life of the child may be saved by version or applications of forceps The cause of acute parenchymatous nephritis during the course of pregnancy is very ably treated by Professor Bartels, found in Ziemssen's Encyclopedia of the Practice of Medicine, vol, XV.

The quality and quantity of the daily secretion of urine by chemical and microscopical examination was pointed out as a valuable diagnostic sign.

When complications are found to exist, the urine of the patient should be daily measured, to ascertain whether the kidneys were performing their functions, and secreting the normal amount of urine.

If the quantity of urine daily secreted was found below the amount, we should endeavor to re-establish the impaired function of the kidneys.

If this effort failed, supplement this loss by increased action of other excretory organs, as recently recommended by Professor Barker.

If the amount of urine is lessening, then induce premature labor and avoid eclampsia; if the child is viable, then it is our imperative duty to induce labor. As a diaphoretic, *Pilocarpine* was mentioned by Thomas. He says there is a class of cases in which the urine is freely secreted, but when, upon boiling with nitric acid, only a small portion solidifies; and in those cases there is no tendency to amaurosis or stertorous respiration at night, both of which he regards as valuable diagnostic signs. In patients where the opposite conditions were found, he believe nothing but premature delivery would prevent convulsions.

Attee employed venesection as a prophylactic.

The subject of excessive vomiting in pregnancy, involving the life of the mother, has recently attracted much attention.

A case of gravid nausea where nutrition was maintained for fifty-two days by rectal alimentation alone, is reported by Dr. Campbell, of Augusta, Georgia. The subject and discussion occupies thirty pages. That the stomach rejected food was supposed to result from ulceration of the cervix.

In the discussion that took place in the French Academy of Medicine, this question arose: Is it ever justifiable to induce abortion in cases of excessive vomiting? The ultimate decision of the discussion was of a mixed character. Some striking instances are recorded where pregnant women died from the effects of vomiting. Principles and Practice of Obstetrics, by Bedford. Flint on rectal alimentation (see American Practitioner, January, 1878) does not once refer to nausea and inanition of pregnancy.

But he states that life may be sustained indefinitely, solely by rectal introduction of aliments, from three weeks to five years.

Cazeaux and Hodge make no reference whatever to rectal alimentation, but in such cases employ abortion. In this case, soda water, champagne, "Potion de Rivere," and oxalate of cerium were given; subcutaneous injections of morphine and atropia to quiet nervousness, but all in vain. The family insisted upon having abortion produced, but upon consulting with other physicians, it was not deemed practicable. In due time natural abortion ensued, and a dead fœtus of three and a half months was expelled. Contrary to expectations, the nausea continued for fourteen days after the delivery; rectal alimentation was kept up for that time, and for a much longer period.

The patient gained rapidly in flesh and color. His method of administering nutriment was ten ounces of beef tea twice a day to each injection—one teaspoonful of Borden's or Leibig's, or sometimes Valentine's condensed preparation of meat.

During the interval of the morning and evening injection, a full goblet of water, not quite cold, was given several hours apart.

Another interesting case of gravid nausea successfully treated for five weeks by rectal aliments, is reported by Dr. Garnett of Washington.

We expressed the opinion that true gastritis will result from a protracted period of uterine irritation, and may constitute one of the conditions imperatively demanding rest. Though many deny that there is any organic change in the mucous membrane of the stomach, yet to my mind it is not probable that in extreme cases a hyperæmia is evoked, and such a condition would aggravate gastric ingestion of aliments.

Rectal alimentation has been termed by different writers, "reversed peristaltic action;" "inverted peristalsis;" and Copeland and Pavy, "anti-peristaltic action" of the muscular coats of the intestines.

Dr. Campbell defines this well-known action as "retro stalsis." The manner in which he accounts for the digestion, absorption, and assimulation of food when placed in the rectum is, that nutrient materials injected into the bowels are carried up out of the rectum, and made to reach that portion of the digestive canal where all the conditions of normal intestinal digestion may be fully met with, and rectal ingestion takes place as well as if it had arrived from the buccal cavity.

This process or vital action of co-ordinated and continuous retro stalsis is to the rectum and small intestines what deglutition is to the esophagus and stomach, and for a more appropriate designation he terms it, "Intestinal inhaustion."

Chadwick proposes in desperate cases to inject with the aspirator through the abdominal walls nutrient fluids and stimulants into the small intestines, "that part of the alimentary canal from which they will be most readily absorbed." (Am. Jour. Obst., vol. VIII, p. 399, November, 1875.)

Dr. Bennett says, since his attention has been attracted to this subject, "he is persuaded" that those gastric disorders and obstinate vomitings are almost all caused by inflammatory ulcerations of the cervix.

Sudden narcotism from opium suppositories, where consciousness was almost wholly suspended for fourteen hours after the removal of ovarian tumors. (American Gynæ. (Trans., vol. III.) The case is reported simply on account of the sudden and almost overwhelming action of the opium employed. For two days the patient had used the suppositories which so suddenly, in the afternoon of the third day, induced such a profound narcotic effect. They were given at regular intervals, and had been carefully prepared by a skillful druggist.

The explanation of this unexpected effect is not perfectly apparent, but the case forms an excellent commentary on Dr. Campbell's paper on rectal alimentation. Was the absorption from the rectum abolished for a time and suddenly renewed? Is opium administered in this manner liable to have a cumulative effect? Or, as is probably the case, were the suppositories crowded into a mass of feces, so as only to reach the absorbing surface of the rectum in a combined mass?